PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXX. For the Year 1780.

PART I.

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LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

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ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations, which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought adviseable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers, as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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APPENDIX.

PHILOSOPHICAL

TRANSACTIONS.

I. *Calculations to determine at what Point in the Side of a Hill its Attraction will be the greatest, &c.*

By Charles Hutton, *LL. D. and F. R. S.*

In a Letter to Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read Nov. 11, 1779.

DEAR SIR, Royal Military Acad. Sept. 21, 1779.

AS the experiment of determining the universal attraction of matter, which you lately conducted with so much accuracy and success, is of so great im-

 portance

portance and curiosity, that it may probably be hereafter repeated by the learned in other countries, and in other situations; and as the utmost precision is desirable in so nice an experiment; I have composed the inclosed paper to determine the best part of a hill for making the observations, so as to obtain the greatest quantity of attraction.

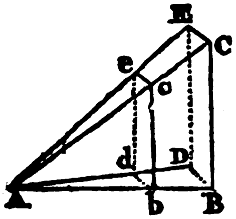
I have no doubt, Sir, that the determination of this point will appear of some consequence in the opinion of one who has the improvement of useful knowledge so much at heart; and if the manner in which it is here made, meet your approbation, I shall desire the favour of your presenting it to the Royal Society.

I have the honour to be, &c.

1. The

1. The great fuccefs of the experiment, lately made by the Royal Society, on the hill Schehallien, to determine the univerfal attraction of matter, and the important confequences that have refulted from it, may probably give occafion to other experiments of the fame kind to be made elfewhere: and as all poffible means of accuracy and facility are to be defired in fo delicate and laborious an undertaking; it has occurred to me that it might not be unufeful to add, by way of fupplement to my paper of calculations relative to the above-mentioned experiment, an inveftigation of the height above the bottom of a hill, at which its horizontal attraction fhall be the greateft; fince that is the height at which commonly the obfervations ought to be made, and fince this beft point of obfervation has never been any where determined that I know of, but has been varioufly fpoken of or gueffed at, it being fometimes accounted at $\frac{1}{3}$, and fometimes at $\frac{1}{2}$ of the height of the hill; whereas from thefe inveftigations it is found to be generally at about only $\frac{1}{4}$ of the altitude from the bottom.

2. Let ABCEDA be part of a cuneus of matter, its fides or faces being the two fimilar right-angled triangles ABC, ADE meeting in the point A, and forming the indefinitely fmall angle BAD. Then of any fection



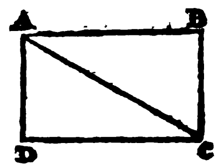
bced, perpendicular to the planes ABD and ADE, the attraction on a body at A in the direction AB, is equal to the constant quantity Ss; where S = sin. <BAC and s = sin. <BAD, to the radius 1.

For, first, since the magnitude of the flowing section is every where as Ab^2, and the attraction of the particles of matter inversely as the same, or as $\frac{1}{\mathrm{A}b^2}$; therefore their product or $\frac{\mathrm{A}b^2}{\mathrm{A}b^2}$ or 1 (a constant quantity) is as the force of attraction of bced.

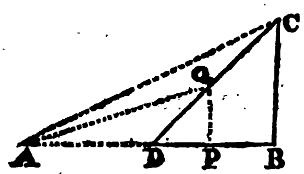
Then to find what that quantity is. Put AB = a, and BC = x; then BD or CE (the distance between the two planes at the distance AB) is = as. Now the force of a particle in the line CE is as $\frac{1}{\mathrm{AC}^2}$ in the direction AC, and therefore it is as $\frac{\mathrm{AB}}{\mathrm{AC}^3}$ in the direction AB; consequently the force of the whole lineola CE in the direction AB is $\frac{\mathrm{AB}.\mathrm{CE}}{\mathrm{AC}^3}$; and therefore the fluxion of the force of the section BCED or $\dot{f}$ is $= \frac{\mathrm{AB}.\mathrm{CE}}{\mathrm{AC}^3}.\dot{\mathrm{BC}} = \frac{a.as.\dot{x}}{\overline{a^2+x^2}|^{\frac{3}{2}}} = \frac{a^2s\dot{x}}{\overline{a^2+x^2}|^{\frac{3}{2}}}$; and the fluent gives $f = \frac{sx}{\sqrt{a^2+x^2}} = s \times \frac{\mathrm{BC}}{\mathrm{AC}} = s\mathrm{S}$ for the attraction itself.

3. To

3. To find now the attraction of the whole right-angled cuneus on a body at A in the direction AB.—— Since the force of each ſection is ss by the laſt article, therefore the force of all the ſections, the number of them being AB or a, is $ass = s \,.\, \text{AB} \,.\, \frac{\text{BC}}{\text{AC}}$ the force of the whole cuneus ABCEDA.



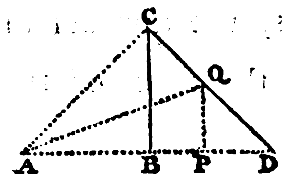
4. To find the attraction of the rectangular part ABCD on A in the direction AB; ABCD being one ſide of the cuneus, and AD its edge.—Put $\text{AD} = \text{BC} = b$, and $\text{AB} = x$. Then, the force of any ſection as BC being always as ss by Art. 2, the fluxion of the force or $\dot{f}$ will be $= ss\dot{x} = s\dot{x} \times \frac{\text{BC}}{\text{AC}} = s\dot{x} \times \frac{b}{\sqrt{b^2+x^2}} = \frac{bs\dot{x}}{\sqrt{b^2+x^2}}$; and the fluent is $f = bs \times$ hyp. log. of $\frac{x+\sqrt{b^2+x^2}}{b} = s \,.\, \text{BC} \times$ hyp. log. $\frac{\text{AB}+\text{AC}}{\text{BC}}$ = the attraction of ABCD.



5. To find the attraction of the right-angled part BCD of a cuneus whoſe edge paſſes through A the place of the body attracted.—Put $\text{AB} = a$, $\text{BC} = b$, $\text{BD} = c$, $\text{DA} = d = a - c$, $\text{DC} = e$, $\text{AC} = g$, and $\text{DP} = x$. Then, the force of any ſection PQ being ſtill as ss, the fluxion of the force of the part DPQ is $\dot{f}$ $= ss\dot{x} = s\dot{x} \times \frac{\text{PQ}}{\text{AQ}} = \frac{bsx\dot{x}}{\sqrt{c^2d^2+2c^2dx+e^2x^2}}$; and the correct fluent

when

when $x = c$ is $f = \frac{bcs}{ee} \times \overline{g - d - \frac{dc}{e} \times \text{hyp. log. } \frac{ee + eg + dc}{de + dc}}$ = the force of a body at A in the direction AB.



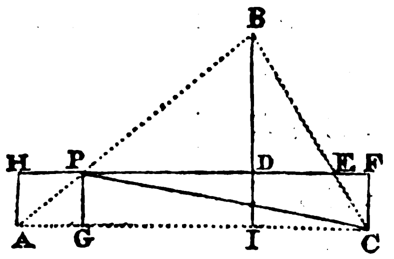
6. To find the attraction of the right-angled part BCD on the point A.—Using here again the same notation as in the last article, we have $\dot{f} = \text{S}s\dot{x} = s\dot{x} \times \frac{\text{PQ}}{\text{AQ}} = \frac{bsx\dot{x}}{\sqrt{c^2d^2 - 2c^2dx + e^2x^2}}$. The correct fluent of which, when $x = c$, is

$$f = \frac{bcs}{ee} \times \overline{g - d + \frac{dc}{e} \times \text{hyp. log. } \frac{ee + eg - dc}{de - dc}}.$$

7. To apply now these premises to the finding of the place where the attraction of a hill is greatest, it will be necessary to suppose the hill to have some certain figure. That position is most convenient for observing the attraction, in which the hill is most extended in the east and west direction. Supposing then such a position of a hill, and that it is also of a uniform height and meridional section throughout; the point of observation must evidently be equally distant from the two ends. But instead of being only considerably extended, I shall suppose the hill to be indefinitely extended to the east and to the west of the point of observation, in order that the investigation may be mathematically true, and yet at the same time sufficiently exact for the before-said limited extent also. It will also come nearest to the

I practical

practical experiment, to suppose the hill to be a long triangular prism, so that all its meridional sections may be similar triangles. Let therefore the triangle ABC represent its section by a vertical plane passing through the meridian, or one side of an indefinitely thin cuneus whose edge is in PG; or rather PBCGP the side of one cuneus, and PAG the side of another, their common edge being the line PG perpendicular to the base AC; P being the required point in the side AB where the attraction of the section ABC, or indefinitely thin cuneus, shall be greatest in a direction parallel to the horizon AC. And then from the foregoing suppositions, it is evident that in whatever point of AB the attraction of ABC is greatest, there also will the attraction of the whole hill be the greatest.



8. Now draw HPDEF parallel to AC; and AH, PG, BI, CF, perpendicular to the same. Then it is evident that at the point P, in the direction PF, the attraction of PBCGP is affirmative, and that of PAG negative. But PBCGP is = PBD + BDE + PFCG − EFC; and PAG = PHAG − PHA. Therefore the attractions of PBD, BDE, PFCG, PHA, are affirmative; and those of EFC, PHAG, negative.

Put

Put now $BI = a$, $AI = b$, $IC = c$, $AB = d$, $BC = e$, $AC = g = b + c$, and $PG = x$, the altitude of the point P above the bottom. Also let $s =$ the sine of the indefinitely small angle of the cuneus to rad. 1; and $q^2 = \sqrt{\overline{a^2g^2 - 2abgx + d^2x^2}}$.

Then by Art. 3, the attraction

$$\text{of} \left\{ \begin{array}{l} PBD \text{ is } s \,.\, PD \,.\, \frac{BD}{BP} = sb \times \frac{a-x}{d}, \\ PHA \text{ is } s \,.\, PH \,.\, \frac{PG}{PA} = sb \times \frac{x}{d}. \end{array} \right\}$$

By Art. 4, the attraction

$$\text{of} \left\{ \begin{array}{l} PFCG \text{ is } s \,.\, PG \times \text{h. l. } \frac{PF+PC}{PG} = sx \times \text{h. l. } \frac{ag-bx+qq}{ax}, \\ PGAH \text{ is } s \,.\, PG \times \text{h. l. } \frac{PH+PA}{PG} = sx \times \text{h. l. } \frac{b+d}{a}. \end{array} \right\}$$

By Art. 5, the attraction of EFC is

$$s \,.\, \frac{EF \,.\, FC}{EC^2} \times \overline{PC - PE - \frac{PE \,.\, EF}{EC} \times \text{h. l. } \frac{EC^2 + EC \,.\, PC + PE \,.\, EF}{PE \,.\, EC + PE \,.\, EF}}$$

$$= \frac{sc}{ee} \times \overline{qq - g \,.\, \overline{a - x} - gc \,.\, \frac{a-x}{e} \times \text{h. l. } \frac{acg + eqq + aax - bcx}{g \,.\, \overline{c+e} \,.\, \overline{a-x}}}.$$

Lastly by Art. 6, the attraction of BDE is

$$s \,.\, \frac{BD \,.\, DE}{BE^2} \times \overline{PB - PE + \frac{PE \,.\, DE}{BE} \times \text{h. l. } \frac{BE^2 + BE \,.\, BP - PE \,.\, DE}{PE \,.\, BE - PE \,.\, DE}}$$

$$= \frac{sc}{ee} \,.\, \overline{a - x} \times \overline{d - g + \frac{cg}{e} \times \text{h. l. } \frac{ee + de - cg}{eg - cg}}.$$

These quantities being collected together with their proper signs, and contracted, we have

$$s \times \left\{ \begin{array}{l} \frac{ab}{d} + c \,.\, \frac{ad - qq - dx}{ee} + x \times \text{hyp. log. } \frac{ag + qq - bx}{\overline{b+d} \,.\, x} + \\ \frac{ceg \,.\, \overline{a-x}}{e^3} \times \text{h. l. } \frac{\overline{ee + de - cg} \,.\, \overline{acg + eqq + a^2x - bcx}}{gg \,.\, \overline{ee - cc} \,.\, \overline{a-x}} \end{array} \right\}$$

for the whole attraction in the direction PE.

9. Having now obtained a general formula for the meaſure of the attraction in any ſort of triangle, if the particular values of the letters be ſubſtituted which any practical caſe may require, and the fluxion of this attraction be put = o, the root of the reſulting equation will be the required height from the bottom of the hill.

10. But for a more particular ſolution in ſimpler terms, let us ſuppoſe the triangle ABC to be iſoſceles, in which caſe we ſhall have $d = e$, and $g = 2b = 2c$, and then the above general formula will become

$$S \times \left\{ \begin{array}{l} \frac{2ad - qq - dx}{dd} b + x \times \text{h. l.}\ \frac{2ab + qq - bx}{\overline{b + d} \,.\, x} \\ + \frac{a - x}{d^3} \,.\, 2b^3 \times \text{h. l.}\ \frac{2ab^2 + dq^2 - \overline{b^2 - a^2} \,.\, x}{2b^2 \,.\, \overline{a - x}} \end{array} \right\}$$

for the value of the attraction in the caſe of the iſoſceles triangle, where q^2 is $= \sqrt{4a^2b^2 - 4ab^2x + d^2x^2}$. And the fluxion of this expreſſion being equated to o, the equation will give the relation between a and x for any values of b and d, by a proceſs not very troubleſome.

11. Now it is probable that the relation between a and x, when the attraction is greateſt, will vary with the various relations between b and d, or between b and a. Let us therefore find the limits of that relation, between which it may always be taken, by uſing two particular extreme caſes, the one in which the hill is

very steep, and the other in which it is very flat, or a very small in respect of b or d.

12. And first let us suppose the triangular section to be equilateral; in which case the angle of elevation is 60°, which being a degree of steepness that can scarcely ever happen, this may be accounted the first extreme case. Here then we shall have $d = 2b = \frac{1}{3}a\sqrt{3}$, and the formula in Art. 10, will become $s \times : \frac{2a-r-x}{2} + x \times$ h. l. $\frac{2a+2r-x}{3x} + \frac{a-x}{4} \times$ h. l. $\frac{a+2r+x}{a-x}$ for the value of the attraction in the case of the equilateral triangle, in which r is $= \sqrt{a^2 - ax + x^2}$.

13. Or if we take $x = na$, where n expresses what part of a is denoted by x, the last formula will become $sa \times : 1 - \frac{1}{2}n - \frac{1}{2}\sqrt{1-n+n^2} + n \times$ h. l. $\frac{2-n+2\sqrt{1-n+n^2}}{3n}$ + $\frac{1-n}{4} \times$ h. l. $\frac{1+n+2\sqrt{1-n+n^2}}{1-n}$ for the case of the equilateral triangle.

14. To find the maximum of the expression in the last article, put its fluxion = 0, and there will result this equation $1 + \frac{1+n}{\sqrt{1-n+n^2}} = 2$ h. l. $\frac{2-n+2\sqrt{1-n+n^2}}{3n}$ − $\frac{1}{2}$h. l. $\frac{1+n+2\sqrt{1-n+n^2}}{1-n}$; the root of which is $n = \cdot 251999$. Which shews that, in the equilateral triangle, the height from the bottom to the point of greatest attraction, is only

only $\frac{1}{500}$th part more than $\frac{1}{4}$ of the whole altitude of the triangle. And this is the limit for the ſteepeſt kind of hills.

15. Let us find now the particular values of the meaſure of attraction ariſing by taking certain values of n varying by ſome ſmall difference, in order to diſcover what part of the greateſt attraction is wanting by obſerving at different altitudes.

16. And firſt uſing the value of n (·251999) as found in the 14th article, the general formula in Art. 13, gives $sa \times 1{\cdot}0763700$ for the meaſure of the greateſt attraction.

17. If $n = \frac{3}{10}$, or $x = \frac{3}{10}a$; the ſame formula gives $\frac{sa}{20} \times : 17 - \sqrt{79} + 6\,\text{h. l.}\ \frac{17 + 2\sqrt{79}}{9} + \frac{7}{2}\text{h. l.}\ \frac{13 + 2\sqrt{79}}{7} = sa \times 1{\cdot}0702512$ for the attraction at $\frac{3}{10}$ of the altitude, which is ſomething leſs than the other.

18. If $n = \frac{4}{10} = \frac{2}{5}$; the formula gives $\frac{sa}{20} \times : 16 - \sqrt{76} + 8\,\text{h. l.}\ \frac{8 + \sqrt{76}}{6} + 3\,\text{h. l.}\ \frac{7 + \sqrt{76}}{3} = sa \times 1{\cdot}0224232$ for the attraction at $\frac{4}{10}$ or $\frac{2}{5}$ of the altitude; leſs again than the laſt was.

19. If $n = \frac{5}{10} = \frac{1}{2}$; the formula gives $\frac{1}{4}sa \times : 3 - \sqrt{3} - 2\,\text{h. l.}\ 3 + \frac{5}{2}\text{h. l.}\ \overline{3 + 2\sqrt{3}} = sa \times {\cdot}9340963$ for the attraction at half way up the hill; ſtill leſs again than the laſt.

 20. If

20. If $n = \frac{6}{10} = \frac{3}{5}$; the formula gives $\frac{sa}{20} \times : 14 - \sqrt{76} + 12\,\text{h. l.}\, \frac{7+\sqrt{76}}{9} + 2\,\text{h. l.}\, \frac{8+\sqrt{76}}{2} = sa \times \cdot 8109843$ for the attraction at $\frac{6}{10}$ or $\frac{3}{5}$ of the altitude from the bottom; being still less than the last was. And thus the quantity of attraction is continually less and less the higher we ascend up the hill above the ·251999 part, or in round numbers ·252 part of the altitude. Let us now descend, by trying the numbers below ·252; and first,

21. If $n = \cdot 25 = \frac{1}{4}$; the same formula in Art. 13, gives $\frac{1}{8} sa \times : 7 - \sqrt{13} + 2\,\text{h. l.}\, \frac{7+2\sqrt{13}}{3} + \frac{3}{2}\text{h. l.}\, \frac{5+2\sqrt{13}}{3} = sa \times 1 \cdot 0763589$ for the attraction at $\frac{1}{4}$ of the altitude; and is very little less than the maximum.

22. If $n = \frac{2}{10} = \frac{1}{5}$; the formula gives $\frac{1}{10} sa \times : 9 - \sqrt{21} + 2\,\text{h. l.}\, \frac{9+2\sqrt{21}}{3} + 2\,\text{h.l.}\, \frac{3+\sqrt{21}}{2} = \frac{1}{10} sa \times : 9 - \sqrt{21} + 2\,\text{h. l.}\, \frac{23+5\sqrt{21}}{2} = sa \times 1 \cdot 0684622$ for the attraction at $\frac{2}{10}$ or $\frac{1}{5}$ of the altitude; and is something less than at $\frac{1}{4}$ of the altitude.

23. If $n = \frac{1}{10}$; the formula gives $\frac{sa}{20} \times : 19 - \sqrt{91} + 2\,\text{h. l.}\, \frac{19+2\sqrt{91}}{3} + \frac{9}{2}\text{h. l.}\, \frac{11+2\sqrt{91}}{9} = sa \times \cdot 9986188$ for the attraction at $\frac{1}{10}$ of the altitude; still less than the last was. And, lastly,

24. If $n = 0$, or the point be at the bottom of the

 hill;

hill; the formula gives $\frac{1}{4}sa \times \overline{2 + \text{h. l. } 3} = sa \times {}^{\cdot}7746531$ for the attraction at the bottom of the hill; which is between $\frac{2}{3}$ and $\frac{3}{4}$ of the greateſt attraction, being ſomething greater than $\frac{2}{3}$ but leſs than $\frac{3}{4}$ of it.

25. The annexed table exhibits a ſummary of the calculations made in the preceding articles; where the firſt column ſhews at what part of the altitude of the hill the obſervation is made; the ſecond column contains the correſponding numbers which are proportional to the attraction; and the third column ſhews what part of the greateſt attraction is loſt at each reſpective place of obſervation, or how much each is leſs than the greateſt.

$\frac{6}{10}$	8109843	$\frac{1}{4}$
$\frac{5}{10}$	9340963	$\frac{2}{13}$
$\frac{4}{10}$	10224232	$\frac{1}{20}$
$\frac{3}{10}$	10702512	$\frac{1}{180}$
$\frac{212}{1000}$	10763700	0
$\frac{1}{4}$	10763589	$\frac{1}{97851}$
$\frac{2}{10}$	10684622	$\frac{1}{134}$
$\frac{1}{10}$	9986188	$\frac{1}{14}$
0	7746531	$\frac{2}{7}$

26. Having now ſo fully illuſtrated the caſe of the firſt extreme, or limit, let us ſearch what is the limit for the other extreme, that is, when the hill is very low or flat. In this caſe b is nearly equal to d, and they are both very great in reſpect of a; conſequently the formula for the attraction in Art. 10, will become barely $s \times : x \times \text{h. l. } \frac{2a-x}{x} + 2\,.\,\overline{a-x} \times \text{h. l. } \frac{2a-x}{a-x}$; the fluxion of which being put $= 0$, we obtain $0 = \text{h. l. } \frac{2a-x}{x} - 2\,\text{h. l. } \frac{2a-x}{a-x} = \text{h. l. } \frac{2a-x}{x} - \text{h. l. } \overline{\frac{2a-x}{a-x}}\big|^{2} = \text{h. l. } \frac{\overline{a-x}|^{2}}{x\,.\,\overline{2a-x}}$; hence therefore $\overline{a-x}|^{2} = x\,.\,\overline{2a-x}$, and $x = a \times \overline{1 - \sqrt{\frac{1}{2}}} = {}^{\cdot}2929a$.

·2929*a*. Which ſhews that the other limit is $\frac{29}{100}$; that is, when the hill is extremely low, the point of greateſt attraction is at $\frac{29}{100}$ of the altitude, like as it is at $\frac{25}{100}$ when the hill is very ſteep. And between theſe limits it is always found, it being nearer to the one or the other of them, as the hill is flatter or ſteeper.

27. Thus then we find that at $\frac{1}{4}$ of the altitude, or very little more, is the beſt place for obſervation, to have the greateſt attraction from a hill in the form of a triangular priſm of an indefinite length. But when its length is limited, the point of greateſt attraction will deſcend a little lower; and the ſhorter the hill is, the lower will that point deſcend. For the ſame reaſon, all pyramidal hills have their place of greateſt attraction a little below that above determined. But if the hill have a conſiderable ſpace flat at the top, after the manner of a fruſtum, then the ſaid point will be a little higher than as above found. Commonly, however, $\frac{1}{4}$ of the altitude may be uſed for the beſt place of obſervation, as the point of greateſt attraction will ſeldom differ ſenſibly from that place. And when uncommon circumſtances may produce a difference too great to be intirely neglected, the obſerver muſt exerciſe his judgment in gueſſing at the neceſſary change he ought to make in the place of obſervation, ſo as to obtain the beſt effect which the concomitant circumſtances will admit of.

II. *An Account of ſome new Experiments in Electricity, with the Deſcription and Uſe of two new Electrical Inſtruments. By Mr.* Tiberius Cavallo, *F. R. S. communicated by the Preſident.*

Read Nov. 25, 1779.

PROFESSOR LICHTENBERG, of Gottingen, ſome time ago made an experiment upon the electrophorus, an account of which was firſt received in London towards the latter end of the year 1777. The phenomena attending the experiment are very entertaining and various, but I do not know that any perſon ever offered a ſatisfactory explanation of them. The author himſelf, in his paper entitled "*De nova methodo* "*naturam ac motum Fluidi Electrici inveſtigandi Com-* "*mentatio prior,*" wherein he gives an account of the experiment, does not attempt any explanation of it; contenting himſelf with the account only of various particulars attending it. In brief, the experiment is as follows:

The electrophorus, that is, a plate of ſome reſinous ſubſtance, as ſulphur, roſen, gum-lac, &c. is firſt excited, either by rubbing, or otherwiſe; then a piece of metal of any ſhape at pleaſure, as, for inſtance, a three-legged compaſs, a piece of braſs tube, or the like, is ſet upon the electrophorus; and to this piece of metal, ſo placed, a ſpark is given of the electricity contrary to that of the plate. This done, the piece of metal is removed by means of a ſtick of ſealing-wax, or other electric; and ſome powder of roſen, kept in a linen bag, is ſhaken upon the electrophorus. This powder will be found to fall about thoſe points upon the plate which the piece of metal touched, forming ſome radiated appearances much like the common repreſentations of ſtars; at the ſame time upon the greateſt part of the plate, that is, in all parts except where the ſtars are formed, there is hardly any powder at all. Now it is to be remarked, that if the plate be excited negatively, and the ſpark given to the metal ſet upon it, be poſitive, the appearance will be as above deſcribed; but if on the contrary the plate is poſitive, and the ſpark is negative, then the powder of roſen will be found to fall upon thoſe parts of the plate, which in the other caſe are left uncovered; and to leave the ſtars clean: in ſhort, it will do juſt the reverſe of what it did in the other caſe: or, in other words,

words, the powder of rosin will be attracted by those parts only of the electrophorus, which are electrified positively.

When I first observed these phenomena, I thought that there was no apparent reason why the powder of rosin should be attracted by those parts of the electrophorus, which are in a positive state of electricity, and not by those, which are negative. The two electricities are certainly contrary to one another; but either of them attracts a non-electrified body. On this consideration I thought, that the experiment could be explained only upon the supposition, that the powder of rosin, on its falling from the linnen bag, was actually electrified negatively; in which case it would have been easy to account for the phenomena upon the well known principle of bodies attracting each other when they are contrarily electrified; and repelling one another when they are possessed of the same kind of electricity.

In order to try the reality of my supposition by experiments, I insulated a brass plate upon a glass stand, and connected a very sensible electrometer with it; and then began shaking the powder of rosin upon it, in the same manner as I had done upon the electrophorus, and in a few seconds of time had the pleasure to see the electro-

meter diverge with a very manifeſt degree of negative electricity, anſwering my expectations exactly.

The explanation of the ingenious Profeſſor LICHTENBERG's experiment now became very eaſy and natural; for the powder of roſin, being actually electrified negatively, could not be attracted, except by thoſe parts of the electrophorus, which are in a contrary ſtate, that is, electrified poſitively.

It is obſerved, that powder of roſin anſwers better for this experiment than the powders of other ſubſtances; and accordingly I find, that this powder, when ſhaken upon the inſulated braſs plate, ſhews a ſtronger degree of electricity than the other powders. Indeed the electricity of the powder of roſin, not only when ſhaken upon the braſs plate in the manner above mentioned, but ſimply let fall upon it from a piece of paper, a ſpoon, &c. is remarkably great; half an ounce of it being ſufficient to make the threads of the electrometer diverge as much as they poſſibly can.

This diſcovery not only affords an eaſy explanation of Profeſſor LICHTENBERG's experiment upon the electrophorus, but ſhews a method of exciting powders, which has long been a *deſideratum* in the ſcience of electricity. The method is as follows; inſulate a metal plate upon an electric ſtand, and connect with it a cork-ball electrometer;

3 then

then the powder required to be tried being held in a ſpoon, or other thing, about ſix inches above the plate, is to be let fall gradually upon it. In this manner the electricity acquired by the powder, being communicated to the metal plate and to the electrometer, is rendered manifeſt by the divergence of the threads, and its quality may be aſcertained in the uſual manner. See fig. 4.

It muſt be obſerved, that if the powder is of a conducting nature, like the amalgam of metals, ſand, &c. it muſt be held in ſome electric ſubſtance, as a glaſs phial, a plate of ſealing wax, or the like. The metal ſpoon that holds the powder may alſo be inſulated; in which caſe, after the experiment, the ſpoon will be found poſſeſſed of an electricity contrary to that of the powder.

In performing theſe experiments care muſt be had to render the powders, and whatever they are held in, as free from moiſture as poſſible, it being ſometimes neceſſary to make them very warm, otherwiſe the experiments are apt to fail.

The following are the particulars I have obſerved with this new method, which, however, are neither numerous nor often repeated; but they may ſuffice to excite the curioſity of thoſe perſons who have leiſure and the opportunity of repeating them more at large, and in a greater variety.

Powder of roſin, whether it be let fall from paper, glaſs, or a metal ſpoon, electrifies the plate ſtrongly negative; the ſpoon, if inſulated, remaining ſtrongly poſitive. Flower of ſulphur produces the ſame effect, but in a leſs degree.

Pounded glaſs, let fall from a piece of paper, made dry and warm, electrifies the plate negatively, but not in ſo ſtrong a degree as roſin. If it is let fall from a braſs cup, it electrifies the plate poſitively, but in a very ſmall degree.

Steel filings, let fall either from a glaſs phial or paper, electrified the plate negatively; but braſs filings, treated in the ſame manner, electrified the plate poſitively.

The amalgam of tin-foil and quickſilver, gunpowder, or very fine emery, electrify the plate negatively when they are let fall upon it from a glaſs phial.

Quickſilver poured from a glaſs phial electrifies the plate poſitively.

Soot from the chimney, or aſhes from common pit coals mixed with ſmall cinders, electrify the plate negatively, when they are let fall from a piece of paper.

Deſcription

Description of the improved atmospherical electrometer.

Fig. 2. is a geometrical representation of my new atmospherical electrometer in its real size. This instrument, the first hint of which I received from my ingenious friend THOMAS RONAYNE, Esq. after various trials, I brought to the present state of perfection as long ago as the year 1777; and immediately after several of them were made after my pattern by Mr. ADAMS, philosophical instrument maker in Fleet-street. The great difficulty attending the construction of this instrument has long dissuaded my publishing any description of it; nor had I ever troubled the Royal Society with it if the observations of several of my friends, who have used it in England and abroad, joined to my own repeated experiments, had not indisputably confirmed its superiority over any other instrument of that kind. Its particular advantages are, 1. The smallness of the size. 2. Its being always ready for experiments, without fear of entangling the threads, or having an equivocal result by the sluggishness of its motion. 3. Its being not disturbed by the wind. 4. Its great sensibility: and 5, its keeping the communicated electricity longer than any other electrometer hitherto used.

The

The principal part of this inſtrument is a glaſs tube CDMN, cemented at the bottom into the wooden piece AB, by which part the inſtrument is to be held when uſed for the atmoſphere, and it alſo ſerves to ſcrew the inſtrument into its wooden caſe ABO, fig. 1. when it is not to be uſed. The upper part of the tube CDMN is ſhaped tapering to a ſmaller extremity, which is entirely covered with ſealing wax melted by heat, and not diſſolved in ſpirits. Into this tapering part a ſmall tube is cemented, which, with its under extremity, touches the flat piece of ivory H, faſtened to the tube by means of cork. The upper extremity of the wire projects about a quarter of an inch above the tube, and ſcrews into the braſs cap EF, which cap is open at the bottom, and ſerves to defend the waxed part of the inſtrument from the rain, &c. In fig. 3. this braſs cap is repreſented as tranſparent, in order to ſhew its internal ſhape, and the manner in which it is ſcrewed to the wire projecting above the tube L. The ſmall tube L and the upper extremity of the large tube CDMN appear like one continued piece, on account of the ſealing wax which covers them both. The conical corks P of this electrometer, which by their repulſion ſhew the electricity, &c. are as ſmall as they can conveniently be made, and they are ſuſpended by exceedingly fine ſilver wires: theſe wires are ſhaped in a ring

ring at the top, by which they hang very loosely to the flat piece of ivory H, which has two holes for that purpose. By this method of suspension the friction is lessened almost to nothing, and thence the instrument is sensible of a very small degree of electricity. IM and KN are two narrow slips of tin-foil stuck to the inside of the glass CDMN, and communicating with the wooden bottom AB; they serve to convey off that electricity, which, when the corks touch the glass, is communicated to it, and being accumulated might disturb the free motion of the corks.

In regard to its use, this instrument may be used to observe the artificial as well as the atmospherical electricity. When it is to be used for artificial electricity, this electrometer is set upon a table or other convenient support; then it is electrified by touching the brass cap EF with an electrified body, which electricity will sometimes be preserved for more than an hour; in this state, if any electrified substance is brought near the cap EF, the corks of the electrometer by their converging or diverging more, will shew the species of that body's electricity.

Before we proceed farther, it is necessary to remark, that the communication of any electricity to this electrometer, by means of an excited electric, for instance, a piece of sealing wax (which we suppose as always electrified

trified negatively) is not very readily done in the ufual manner, becaufe of the cap EF being well rounded, and free from points or fharp edges. By the approach of the wax the electrometer will be caufed to diverge; but as foon as the wax is removed, the wires immediately collapfe. The beft method to electrify it is, to bring the excited wax fo near the cap that one or both the corks may touch the fide of the bottle CDMN; after which they will foon collapfe and appear unelectrified. If now the wax is removed, they will again diverge and remain electrified pofitively. In this operation the wax does not impart any of its electricity to the electrometer, but only acts by means of its atmofphere, *viz.* when the excited wax is firft brought near the brafs cap EF (agreeable to the well known law of electricity, and according to Dr. FRANKLIN's hypothefis) it determines the electric fluid, naturally belonging to the corks, towards the cap; hence the corks repel each other. Now if in this ftate they touch the fides of the glafs CDMN, they acquire from it a quantity of electric fluid equal to that, which, by the action of the excited wax, was driven towards the cap, confequently they collapfe and appear unelectrified. Notwithftanding this appearance, the cap is actually overcharged, fo that when the wax is removed, the overplus of the electric fluid, which the corks had acquired from

from the glaſs, and tin-foil ſtuck upon it, and which was crouded upon the cap, becauſe the negative atmoſphere of the wax now diffuſes itſelf equally through the cap, the wires, the corks, &c.; and, therefore, the corks repel each other with poſitive electricity.

If, inſtead of the ſealing wax excited negatively, an electric poſſeſſed of poſitive electricity be uſed, the electrometer acquires the negative electricity, and the explanation, *mutatis mutandis*, is the ſame as above.

By conſidering this remark it will appear, that when this electrometer is electrified, either poſitively or negatively, and an electrified body is brought towards the braſs cap; the electricity of that body will be of the ſame kind with that of the electrometer, if the corks increaſe their divergency; but it will be of the contrary kind, if the corks approach one another.

When this inſtrument is to be uſed to try the electricity of the fogs, the air, the clouds, &c. the obſerver is to do nothing more than to unſcrew it from its caſe, and, holding it by the bottom AB, to preſent it to the open air a little above his head, ſo that he may conveniently ſee the corks P, which will immediately diverge if there is any electricity; the kind of which, that is, whether poſitive or negative, may be aſcertained by bringing an excited

 piece

piece of ſealing wax, or other electric, towards the braſs cap EF.

It is, perhaps, unneceſſary to remark, that this obſervation muſt be made in an open place, as the roads out of town, the fields, the top of a houſe, &c.

I have often made uſe of this electrometer in the roads between Iſlington and London, and by means of it I have confirmed the obſervation of Mr. RONAYNE, who firſt diſcovered the electricity of the fogs, as is teſtified by a paper of his publiſhed in the Philoſophical Tranſactions, and who has remarked, that a fog is very rarely not electrified; and that in clear froſty weather the air is conſtantly electrified.

Promiſcuous Experiments.

I. Having had frequent occaſion to obſerve how difficult it is to deprive ſealing wax of its electricity entirely, after that it has been well excited, I had the curioſity to try whether water could effect it. In order to that, I tied a ſtick of ſealing wax to a ſilk ſtring about a yard long, and after having excited it very powerfully with flannel, I plunged it in a tin veſſel full of water, and immediately drawing

drawing it out, brought a very accurate electrometer near it, and obſerved, that at firſt it ſhewed no ſign of electricity; but in about half a minute's time it manifeſted a ſmall but very ſenſible degree of negative electricity. A glaſs tube, treated in the ſame manner, was deprived of all its electricity by the water.

II. I have often remarked, that after having excited a glaſs tube with the amalgamated rubber in the uſual manner, the part of it which had been under my hand was negative. This minus ſtate was ſtill more conſpicuous when I graſped with my hand the part next above, *viz.* part of that which had been excited poſitively by rubbing. In the ſame manner, when I excite a ſtick of ſealing wax, by rubbing it with flannel, I often find, that the part of it which I hold in my hand is in a contrary ſtate of electricity, *viz.* poſitive.

III. Being deſirous of trying the conducting power of the effluvia of burning bodies in a manner more ſatiſfactory than had hitherto been done, I contrived an inſtrument for that purpoſe, which is repreſented in fig. 5.[(a)].

The handle of it AB is a glaſs tube, into the extremity B of which a wire EI, and a ſmaller glaſs tube BC, are cemented

(*a*) This inſtrument is juſt double the ſize of the figure.

mented by means of ſealing wax. From the extremity of this ſmall tube another wire FG proceeds, which, as well as the wire EI, is bent at the top, ſo that the extremities of both wires EF may be about one tenth of an inch from one another. GH is a ſmall wire faſtened to the wire FG, and to the extremity of the handle, ſo that when the inſtrument is held in one's hand, this wire touches the hand. K is a ſmall cork-ball electrometer, which, when the inſtrument is to be uſed, is affixed to the pin D, which proceeds from the wire IE. When experiments are to be tried with this inſtrument, the electrometer K muſt be affixed to the pin D, which proceeds from the wire IE, and muſt be electrified ſo that the cork balls may diverge as far as poſſible: this done, the extremities EF of the wires are brought within the effluvia that are to be tried, which, if they are of a good conducting nature, will complete the communication between the two wires EF, and diſcharge the electrometer of its electricity, otherwiſe the electrometer will remain electrified for a conſiderable time.

The experiments which I have made with this inſtrument are neither numerous nor ſo often repeated as to be depended upon, excepting one only, which, perhaps, it will be not uſeleſs to mention. I found that the fumes

ariſing

Fig. 2

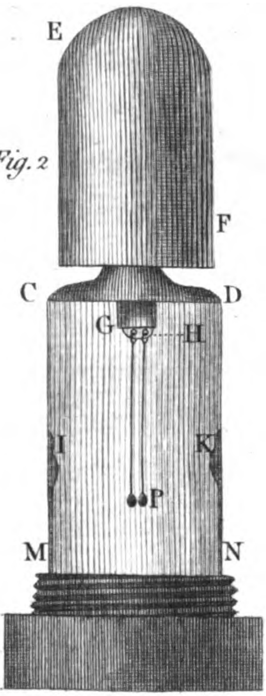


Fig. 3

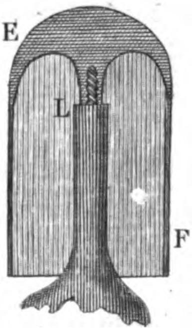


Fig. 4

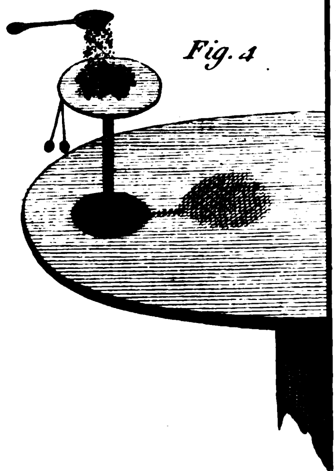
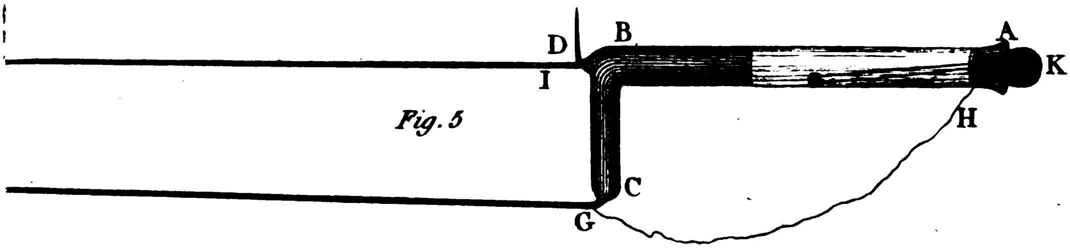


Fig. 5



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ariſing by the action of a lens, from the amalgam of tin-foil and mercury, conducted ſo badly, that the electrometer loſt its electricity in a time very little leſs than it would have done without any fumes whatever.

Little St. Martin's Lane,
Nov. 4, 1779.

III. *A new Method of assaying Copper Ores.* *By* George Fordyce, *M. D. F. R. S.*

Read Nov. 25, 1779.

P R O C E S S.

TAKE 100 grains of the ore, powder it finely, put it into a small matrass or a glass phial, pour upon it half an ounce of nitrous acid, of the strength commonly sold by the name of *aqua fortis*, that is, the pure acid diluted with about four times its weight of water; and half an ounce of muriatic acid sold by the name of spirit of salt; place the vessel in a sand heat, or if you have none, an iron pot or fire shovel with sand may be put over a common fire, and the matrass or phial set in it. Raise a moderate heat, an effervescence will take place for the most part; when this ceases increase the heat till it is renewed, and so proceed till the liquor boils, which is also to be done if no effervescence takes place; boil them together for a quarter of an hour.

Remove the vessel from the fire, and let it cool, then pour into it two ounces of water, shake them together,

and let them ſtand till the liquor is clear; pour the clear liquor into a baſon where it may be preſerved.

Add to the *reſiduum* a freſh half ounce of each of the acids, and proceed again in the ſame manner, mixing the clear liquor with that procured by the firſt proceſs.

The ſame operation is to be repeated until the freſh acids acquire no tinge of blue or green.

Diſſolve half a pound of mild fixed vegetable alkali, commonly ſold by the name of ſalt of tartar, in a quart of water. Purify the ſolution either by filtration, or letting the impurities ſubſide, and decanting the liquor clear into a glaſs veſſel. Pour the ſolution of the alkali ſlowly into the baſon containing the fluid, procured by the former proceſſes, until the whole matter be precipitated from the acids.

Add, by a little at a time, as much vitriolic acid, commonly ſold by the name of oil of vitriol, as will re-diſſolve the whole, or only leave a white powder; if there ſhould be any ſuch powder, which is ſeldom the caſe, it muſt be ſeparated by filtration.

Having the liquor in the baſon now clear, put into it a piece of iron, bright and free from ruſt, and at leaſt an ounce in weight, and leave them together for twenty-four hours, the copper will be found precipitated, principally on the ſurface of the iron, and ſometimes in a powder at the bottom of the baſon.

Decant the fluid from the copper and iron with great care into another bafon, fo that as little as poffible or none of the copper be carried along with it.

Wafh the metals in a pint of water; let them fubfide perfectly, and pour this water into the fecond bafon, with the fame care.

Repeat the wafhing three times. If any copper be found in the fecond bafon, let the wafhings ftand in it for half an hour, fo that the metal fhall fubfide; decant the fluid carefully off, and return the copper into the firft bafon. Pour upon the copper and iron one ounce of vitriolic acid, and two ounces of water; let them ftand together for a quarter of an hour, or until the copper fhall be eafily feparable from the iron. Separate the copper from the iron, taking great care none be loft; the remaining iron may be laid afide. Pour the acid from the copper, after it has fubfided, into the fecond bafon, and wafh the copper with a pint of water, and repeat the wafhing three times, as before directed.

Great care is to be taken, in decanting both the acid and wafhings into the fecond bafon, that none of the copper goes along with them, and left any fhould, they ought to ftand for half an hour in the fecond bafon, and be decanted from it alfo with care, and if any copper is

found at the bottom, it is to be washed and added to the rest.

The copper is now to be dried and weighed, and gives the proportion contained in the ore.

Observations on the above process.

It is about twenty years ago that I contrived some methods of assaying ores, which might avoid tedious and troublesome roastings and fusions in great degrees of heat, which require a dexterity which is only to be acquired by great practice, and which after all form a process that is often various in the result, and seldom shews the substances contained in the ore, excepting the metal. The principles on which these processes depend, as far as regards copper ores, are,

First, Metals are attracted more strongly by acids than by sulphur, with which they are often combined in their ores. In consequence, if a metal be combined with sulphur in an ore, it may be separated by applying an acid, which will unite with the metal, and separate the sulphur. The metal may generally be separated from the acid in its metallic form by means of another metal, which attracts the acid more strongly.

Secondly, Arſenic unites with vitriolic, nitrous, and muriatic acids, forming a corroſion or compound not ſoluble in water; whereas moſt other metals may be united with one of theſe acids, or a mixture of them, ſo as to form a compound ſoluble in water: therefore, if there be arſenic combined with a metal in an ore, if it be diſſolved in ſuch acid diluted with water, the arſenic will fall to the bottom in a white powder or cryſtals, and the ſolution being poured off will contain the metal, which may be ſeparated from the acid by another metal as before.

Thirdly, the calces of metals may be diſſolved in acids, whether they be pure (of which there are few inſtances in ores) or combined with gas, reſpirable air, or other vapours; therefore, if the metal in an ore be in the form of a calx, we may find an acid which will diſſolve it, and we may afterwards precipitate it in its metallic form as before.

Fourthly, When an ore is to be aſſayed, it ſhould be ſeparated from the quartz, ſpars, and other earthy matters, with which it is often mixed, as perfectly as poſſible; however, after all our care there will be often a part of them ſo intimately mixed with the ore, that it cannot be entirely cleared. Many of theſe earthy matters do not diſſolve readily in acids: therefore, if the metal of an ore

ore be diſſolved in an acid, ſo as to form a compound ſoluble in water, the ſolution of the metal may be poured off, leaving ſuch earthy matters behind.

Fifthly, If the earthy matter ſhould diſſolve in the acid, it is ſeldom to be precipitated by a metal: therefore if both earth and metal be diſſolved, on the application of another metal, which attracts the acid more ſtrongly, that which was combined with the acid will be precipitated, and the earth left in the ſolution.

Sixthly, Acids attract the metals with different powers: therefore, if two metals be combined with an acid, if we apply to the ſolution a maſs of that which attracts the acid ſtrongeſt, the other will be precipitated. The maſs being weighed before and after the precipitation, the difference will be the quantity of additional metal diſſolved; if, therefore, we pour off the liquid from the precipitate, and apply another metal, which attracts the acid ſtill more ſtrongly, the ſecond metal will be precipitated, which being weighed, and the weight loſt from the maſs deducted, gives the weight of the ſecond metal. As this principle is of great uſe in inveſtigating the elements of mixed metals, we ſhall give an example. Suppoſe copper and ſilver mixed; diſſolve the whole in pure nitrous acid, properly diluted with water; apply to the ſolution a maſs of copper, the ſilver will be precipitated.

Pour off the ſolution, and waſh the ſilver and undiſſolved copper with water; pour the waſhings into the ſolution; weigh the maſs of copper left, and mark what it has loſt; apply to the ſolution a maſs of iron, the whole copper will be precipitated. Pour off the fluid, and waſh the precipitate carefully, dry it, and weigh it, deduct the weight loſt from the maſs of copper, what remains is the weight of the copper in the mixture; if this weight, together with that of the ſilver, be the weight of the metal originally expoſed to examination, there is no reaſon to ſuſpect any mixture of another metal.

If the metals mixed are unknown, if we can find an acid which will diſſolve them, we may try to make a precipitation with the metal which is loweſt but one in the order of elective attractions, and ſo proceed to the next above it, until we come to the higheſt; and by this means we ſhall obtain all the metals in the maſs.

There are other principles on which I have founded various proceſſes for aſſaying; but theſe are ſufficient for copper ores, all the different known ſpecies of which I have actually aſſayed, and therefore have ventured to offer this proceſs to the conſideration of the Society; firſt, as only requiring an apparatus which can be bought at any apothecary's or chemiſts, and as capable of being performed by a perſon totally unacquainted with

with chemistry, so that any proprietor of an estate, or his servant, may determine if an ore be of copper, and its value; secondly, as affording an assay-master a more perfect manner of determining the value of a copper ore; and, lastly, as a process by which the naturalist may investigate not only the copper in an ore, but its various other contents.

There is but one known species of copper ore in which the copper is not capable of being combined with *aqua regia*, which is blue vitriol, which is sometimes found solid, but more frequently in mineral waters; from this the copper may be precipitated by iron immediately.

We have lately had many opinions published of metals being found in mineral waters combined with various substances. I never examined any mineral water in which I found the metals combined with any other substance but vitriolic acid; and am certain, many authors have been misled by not knowing this property of metallic salts, *viz.* that if we dissolve them in a small proportion of water, or if there be superfluous acid, the solution will remain perfect when exposed to the air; but if the acid be perfectly saturated with the metal, and the proportion of water to the metallic salt be very great, on exposure to the air it is decomposed, the metal precipitating in the form of a calx, and the acid being lost.

This

This may eaſily be-tried by taking common green or blue vitriol, diſſolving an ounce in three ounces of water by boiling, letting them ſtand to cool, and filtering the ſolution. If this ſolution be expoſed to the air it will remain perfect; but if we drop a drop or two of it into a wine glaſs full of water, in a few minutes the tranſparency of the water will begin to be diſturbed, and the metal in a ſhort time will fall down, in a red powder if it be iron, in a blue powder if it be copper.

An hundred grains of the ore is ſufficient to give the copper contained to one hundred part; if greater accuracy be required 1000 grains may be uſed.

The mixture of nitrous and muriatic acid is the moſt proper acid *menſtruum* for copper ores, muriatic acid diſſolving moſt readily the calces of metals, and nitrous acid when they are in their metallic form; a metal in its metallic form being a compound of a pure calx and a ſubſtance, which has been called inflammable air, but which is an oil found out by STAHL to exiſt in metals, and which we would call the oil of metals. The nitrous acid decompoſes this oil, at the ſame time that it acts on the calx itſelf, and leaves it alſo to be acted upon by the muriatic acid.

When copper is combined with ſulphur in an ore, it is in its metallic form; in diſſolving in an acid its oil riſes in

in vapour; or vapours produced by the decomposition of this oil occasion an effervescence.

All the calces of copper I have tried are combined with gas, respirable air, or other vapours, excepting one, which is of a light green colour, brittle, and which breaks smooth like glass; a specimen of it is contained in Dr. HUNTER's museum: this dissolves without effervescence, the others all effervesce. A boiling heat is necessary to render the solution compleat, of which great care is to to be taken.

If there be any sulphur in the ore, it appears quite clear in lumps; a small portion of it, however, is destroyed by the nitrous acid. Earthy matters insoluble in acids, if any, and arsenic, appear in a powder at the bottom. If there be any silver it is mixed with this powder, and is to be extracted by melting it with black flux and litharge, and cupelling in the common way. If there be any gold it may be taken out of the solution by æther.

When the copper is combined with nitrous and muriatic acids, it might be thought sufficient to apply the iron immediately; but it is much more convenient to precipitate it from them, and combine it with vitriolic acid, on account of the convenience of washing the precipitate, which is in a more compacted mass.

If

If there be any calcareous earth diſſolved, the vitriolic acid will combine with it, and form a white powder, which will be left after the copper is re-diſſolved, and muſt be ſeparated carefully from the ſolution.

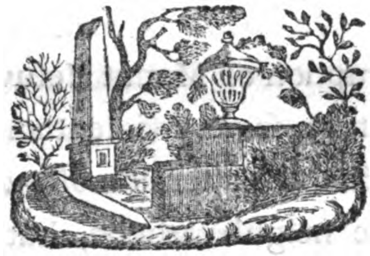
After the precipitation of the copper, it is neceſſary to get rid of the ſalts perfectly before we apply the vitriolic acid, otherwiſe part of the copper would be re-diſſolved.

Vitriolic acid will not diſſolve copper in its metallic form, and may be applied to diſſolve any iron that may be mixed with the precipitate, as well as to looſen copper, which ſometimes adheres to the iron.

The ſolution of the iron muſt be carefully waſhed off from the copper.

There is a criterion by which we may judge certainly if any of the copper be loſt. Let all the waſhings and every thing, except the copper, be put into a veſſel together; pour in ſolution of fixed alkali till no further precipitation takes place; let the precipitate ſubſide, and pour off the liquor; apply to the precipitate ſolution of volatile alkali, ſold by the name of ſpirit of *ſal ammoniac*; ſhake them together, and let them ſtand for an hour; if the ſolution of the alkali acquires a purpliſh blue colour, the proceſs is imperfect, if it does not, it is perfect.

If the procefs be imperfect, which is always for want of care in the decantations, pour in as much vitriolic acid as will diffolve the whole precipitate, apply iron to the folution, the remaining copper will be procured.



IV. *An Account of an Eruption of Mount Vesuvius, which happened in* August, 1779. *In a Letter from Sir* William Hamilton, *K. B. F. R. S. to* Joseph Banks, *Esq. P. R. S.*

Read Dec. 16, 1779.

Naples, October 1, 1779.

S I R,

THE late eruption of Mount Vesuvius was of so singular a nature, so very violent and alarming, that it necessarily attracted the attention of every one, not only in its immediate neighbourhood, but for many miles around; and, consequently, several slight descriptions of it have been already handed about, and some (as I am informed) more accurate and circumstantial are preparing for the press[(a)].

That on which the Abbot BOTTIS is actually employed, by command of his Sicilian Majesty, will undoubtedly be executed with the same accuracy, truth,

(a) The inhabitants of this great city in general give so little attention to Mount Vesuvius, though in full view of the greatest part of it, that I am well convinced many of its eruptions pass totally unnoticed by at least two thirds of them.

and precifion, as have rendered that author's former publications upon the fubject of Mount Vefuvius fo univerfally and defervedly efteemed.

Such a publication, executed with magnificence in the royal printing office, may, perhaps, render every other account of the late eruption fuperfluous: neverthelefs, I fhould think myfelf in fome degree guilty of a neglect towards the Royal Society, who have done fo much honour to my former communications, if I did not, through the refpectable canal of its worthy prefident, and my good friend, fimply relate to them fuch remarkable circumftances as attended the late tremendous explofions of Mount Vefuvius, and as either came immediately under my own infpection, or have been related to me by fuch good authority as cannot be called in queftion.

Since the great eruption of 1767, of which I had the honour of giving a particular account to the Royal Society, Vefuvius has never been free from fmoke, nor ever many months without throwing up red-hot fcoriæ, which, increafing to a certain degree, were ufually followed by a current of liquid lava, and except in the eruption of 1777, thofe lavas broke out nearly from the fame fpot, and ran much in the fame direction, as that of the famous eruption of 1767.

No lefs than nine fuch eruptions are recorded here fince the great one abovementioned, and fome of them were confiderable. I never failed vifiting thofe lavas whilft they were in full force, and as conftantly examined them and the crater of the volcano after the ceafing of each eruption (b).

It would be but a repetition of what has been defcribed in my former letters on this fubject, were I to relate my remarks on thofe different expeditions. The lavas, when they either boiled over the crater, or broke out from the conical parts of the volcano, conftantly formed channels as regular as if they had been cut by art down the fteep part of the mountain, and, whilft in a ftate of perfect fufion, continued their courfe in thofe channels, which were fometimes full to the brim, and at other times more or lefs fo, according to the quantity of matter in motion.

Thefe channels, upon examination after an eruption, I have found to be in general from two to five or fix feet

(b) The laft vifit to the crater of Vefuvius, which was in the month of May, 1779, was my fifty-eighth, and to be fure I have been four times as often on parts of the mountain, without climbing to its fummit, and after all am not afhamed to own, that I comprehend very little of the wonders I have feen in this great laboratory of nature; yet there have been naturalifts of fuch a wonderful penetrating genius, as to have thought themfelves fufficiently qualified to account for every hidden phenomenon of Vefuvius, after having, literally fpeaking, given the volcano *un coup d'œil*.

wide,

wide, and feven or eight feet deep. They were often hid from the fight by a quantity of *fcoriæ* that had formed a cruft over them, and the lava having been conveyed in a covered way for fome yards, came out frefh again into an open channel. After an eruption I have walked in fome of thofe fubterraneous or covered galleries which were exceedingly curious, the fides, top and bottom, being worn perfectly fmooth and even in moft parts by the violence of the currents of the red-hot lavas, which they had conveyed for many weeks fucceffively; in others, the lava had incrufted the fides of thofe channels with fome very extraordinary *fcoriæ:* beautifully ramified white falts [(c)], in the form of dropping ftalactites, were alfo attached to many parts of the cieling of thofe galleries. It is imagined here, that the falts of Vefuvius are chiefly ammoniac, though often tinged with green, deep, or pale yellow, by the vapour of various minerals.

In the month of May laft, there was a confiderable eruption of Mount Vefuvius, when I paffed a night on the mountain in the company of one of my countrymen, as eager as myfelf in the purfuit of this branch of natural hiftory [(d)].

(c) I fent a large fpecimen of this curious volcanic production to the Britifh Mufeum laft year.

(d) Mr. BOWDLER, of Bath.

We

We ſaw the operation of the lava, in the channels as abovementioned, in the greateſt perfection; but it was, indeed, owing to our perſeverance, and ſome degree of reſolution. After the lava had quitted its regular channels, it ſpread itſelf in the valley, and, being loaded with *ſcoriæ*, ran gently on, like a river that had been frozen, and had maſſes of ice floating on it: the wind changing when we were cloſe to this gentle ſtream of lava, which might be about fifty or ſixty feet in breadth, incommoded us ſo much with its heat and ſmoke, that we muſt have returned without having ſatisfied our curioſity, had not our guide [(e)] propoſed the expedient of walking acroſs it, which, to our aſtoniſhment, he inſtantly put in execution, and with ſo little difficulty, that we followed him without heſitation, having felt no other inconveniency than what proceeded from the violence of the heat on our legs and feet; the cruſt of the lava was ſo tough, beſides being loaded with cinders and *ſcoriæ*, that our weight made not the leaſt impreſſion on it, and its motion was ſo ſlow, that we were not in any danger of loſing our balance and falling on it: however, this experiment ſhould not be tried except in caſes of real neceſſity; and I mention it with no other view than to point out a poſ-

(*e*) BARTOLOMEO, the Cyclops of Veſuvius, who has attended me on all my expeditions to the mountain, and who is an excellent guide.

ſibility

fibility of efcaping, fhould any one hereafter, upon fuch an expedition as ours, have the misfortune to be inclofed between two currents of lava.

Having thus got rid of the troublefome heat and fmoke, we coafted the river of lava and its channels up to its very fource, within a quarter of a mile of the crater. The liquid and red-hot matter bubbled up violently, with a hiffing and crackling noife, like that which attends the playing off of an artificial firework, and by the continual fplafhing up of the vitrified matter, a kind of arch or dome was formed over the crevice from whence the lava iffued. It was cracked in many parts, and appeared red-hot within, like an heated oven: this hollowed hillock might be about fifteen feet high, and the lava that ran from under it was received into a regular channel, raifed upon a fort of wall of *fcoriæ* and cinders, almoft perpendicularly, of about the height of eight or ten feet, refembling much an ancient aqueduct.

We then went up to the crater of the volcano, in which we found, as ufual, a little mountain throwing *fcoriæ* and red-hot matter with loud explofions; but the fmoke and fmell of fulphur was fo intolerable, that we were under the neceffity of quitting that curious fpot with the utmoft precipitation.

In

In another of my excurſions to Mount Veſuvius laſt year, I picked up ſome fragments of large and regular cryſtals of cloſe-grained lava or baſalt, the diameter of which, when the priſms were complete, may have been eight or nine inches. As Veſuvius does not exhibit any lava's regularly cryſtallized, and forming what are vulgarly called Giants Cauſeways (except a lava that ran into the ſea near Torre del Greco in 1631, and which in a ſmall degree has ſuch an appearance), this diſcovery gave me the greateſt pleaſure (f).

After this ſlight ſketch of the moſt remarkable events on Veſuvius ſince the year 1767, which I flatter myſelf will not be unacceptable, as it may ſerve to connect what I am going to relate with what has already been communicated to the Society in my former letters on the ſame ſubject, I come to the account of the late eruption, which affords indeed ample matter for curious ſpeculation.

(f) As the fragments of baſalt columns, which I found on the cone of Veſuvius, had been evidently thrown out of its crater, may not lava be more ſubject to cryſtallize within the bowels of a volcano than after its emiſſion, and having been expoſed to the open air? And may not many of the Giants Cauſeways, already diſcovered, be the *nuclei* of volcanic mountains, whoſe lighter and leſs ſolid parts may have been worn away by the hand of time? Mr. FAUJEIS DE ST. FOND, in his curious book lately publiſhed, and intitled, "Recherches ſur les Volcains étcints du Vivarais et du Velay," gives (p. 286.) an example of baſalt columns, that are placed deep within the crater of an extinguiſhed volcano.

As

As many poetical defcriptions of this eruption will not be wanting, I fhall confine mine to fimple matter of fact in plain profe, and endeavour to convey to you, SIR, as clearly and as diftinctly as I am able, what I faw myfelf, and the impreffion it made upon me at the time, without aiming in the leaft at a flowery ftyle.

The ufual fymptoms of an approaching eruption, fuch as rumbling noifes and explofions within the bowels of the volcano, a quantity of fmoke iffuing with force from its crater, accompanied at times with an emiffion of red-hot *fcoriæ* and afhes, were manifeft, more or lefs, during the whole month of July; and towards the end of the month, thofe fymptoms were increafed to fuch a degree as to exhibit in the night-time the moft beautiful fire-works that can be imagined.

Thefe kinds of throws of red-hot *fcoriæ* and other volcanic matter, which at night are fo bright and luminous, appear in broad day-light like fo many black fpots in the midft of the white fmoke; and it is this circumftance that occafions the vulgar and falfe fuppofition, that volcanos burn much more violently at night than in the day-time.

On Thurfday, the 5th of Auguft laft, about two o'clock in the afternoon, I perceived from my villa at Paufilipo in the bay of Naples, from whence I have a

full view of Vesuvius (which is just opposite, and at the distance of about six miles in a direct line from it) that the volcano was in a most violent agitation: a white and sulphureous smoke issued continually and impetuously from its crater, one puff impelling another, and by an accumulation of those, clouds of smoke resembling bales of the whitest cotton. Such a mass of them was soon piled over the top of the volcano as exceeded the height and size of the mountain itself at least four times. In the midst of this very white smoke, an immense quantity of stones, *scoriæ*, and ashes, were shot up to a wonderful height, certainly not less than two thousand feet. I could also perceive, by the help of one of RAMSDEN'S most excellent refracting telescopes, at times, a quantity of liquid lava, seemingly very weighty, just heaved up high enough to clear the rim of the crater, and then take its course impetuously down the steep side of Vesuvius, opposite to Somma. Soon after a lava broke out on the same side from about the middle of the conical part of the volcano, and, having run with violence some hours, ceased suddenly, just before it had arrived at the cultivated parts of the mountain above Portici, near four miles from the spot where it issued.

During this day's eruption, as I have been credibly informed since, the heat was intolerable at the towns of

Somma and Ottaiano; and was likewise sensibly felt at Palma and Lauro, which are much farther from Vesuvius than the former. Minute ashes, of a reddish hue, fell so thick at Somma and Ottaiano, that they darkened the air in such a manner as that objects could not be distinguished at the distance of ten feet. Long filaments of a vitrified matter like spun-glass were mixed and fell with these ashes (g); and the sulphureous smoke was so violent, that several birds in cages were suffocated, the leaves of the trees in the neighbourhood of Somma and Ottaiano were covered with white salts very corrosive. About two o'clock in the afternoon, an extraordinary globe of smoke, of a very great diameter, was distinctly perceived, by many of the inhabitants of Portici, to issue from the crater of Vesuvius, and proceed hastily towards the mountain of Somma, against which it struck and dispersed itself, having left a train of white smoke, mark-

(g) During an eruption of the volcano in the isle of Bourbon in 1766, some miles of country, at the distance of six leagues from that volcano, were covered with a flexible, capillary, yellow glass, some of which were two or three feet long, with small vitrous globules at a little distance one from the other. Count BUFFON shewed me some of this capillary and flexible glass, which is preserved in the Royal Museum at Paris, and which perfectly resembles the filaments of vitrified matter which fell at Ottaiano and in other parts on the borders of Vesuvius during this eruption. SORRENTINO, in his Istoria del Vesuvio, published at Naples in 1734, likewise mentions vitrified matter, like herbs and straw, being found on the ground in the neighbourhood of Vesuvius during an eruption of that mountain in the year 1724.

ing the courſe it had taken: this train I perceived plainly from my villa, as it laſted ſome minutes; but I did not ſee the globe itſelf.

A poor labourer, who was making faggots on the mountain of Somma, loſt his life at this time, and his body not having been found, it is ſuppoſed that, ſuffocated by the ſmoke, he muſt have fallen into the valley from the craggy rocks on which he was at work, and been covered by the current of lava that took its courſe through that valley ſoon after. An aſs, that was waiting for its maſter in the valley, left it very judiciouſly as ſoon as the mountain became violent, and, arriving ſafe home, gave the firſt alarm to this poor man's family.

It was generally remarked, that the exploſions of the volcano were attended with more noiſe during this day's eruption than in any of the ſucceeding ones, when, moſt probably, the mouth of Veſuvius was widened, and the volcanic matter had a freer paſſage. It is certain, however, that the great eruption of 1767 (which in every other reſpect was mild, when compared to the late violent eruption) occaſioned much greater concuſſions in the air by its louder exploſions.

Friday, Auguſt the 6th, the fermentation in the mountain was leſs violent; but, about noon, there was a loud report, at which time it was ſuppoſed, that a portion

of

of the little mountain within the crater had fallen in. At night the throws from the crater increafed, and proceeded evidently from two feparate mouths, which emitting red-hot *fcoriæ*, and in different directions, formed a moft beautiful and almoft continued fire-work.

On Saturday, Auguft the 7th, the volcano remained much in the fame ftate; but, about twelve o'clock at night, its fermentation increafed greatly. The fecond fever-fit of the mountain may be faid to have manifefted itfelf at this time. I was watching its motions from the mole of Naples, which has a full view of the volcano, and had been witnefs to feveral glorious picturefque effects produced by the reflection of the deep red fire, which iffued from the crater of Vefuvius, and mounted up in the midft of the huge clouds, when a fummer ftorm, called here *a tropea*, came on fuddenly, and blended its heavy watry clouds with the fulphureous and mineral ones, which were already like fo many other mountains, piled over the fummit of the volcano; at this moment a fountain of fire was fhot up to an incredible height, cafting fo bright a light, that the fmalleft objects could be clearly diftinguifhed at any place within fix miles or more of Vefuvius.

The black ftormy clouds paffing fwiftly over, and at times covering the whole or a part of, the bright column

of

of fire, at other times clearing away, and giving a full view of it, with the various tints produced by its reverberated light on the white clouds above, in contraſt with the pale flaſhes of forked lightning that attended the *tropea*, formed ſuch a ſcene as no power of art can ever expreſs.

That which followed the next evening was ſurely much more formidable and alarming; but this was more beautiful and ſublime than even the moſt lively imagination can paint to itſelf. This great exploſion did not laſt above eight or ten minutes, after which Veſuvius was totally eclipſed by the dark clouds, and there fell a heavy ſhower of rain.

Some *ſcoriæ* and ſmall ſtones fell at Ottaiano during this eruption, and ſome of a very great ſize in the valley between Veſuvius and the Hermitage. All the inhabitants of the towns at the foot of the volcano were in the greateſt alarm, and preparing to abandon their houſes, had the eruption continued longer.

One of his Sicilian Majeſty's game-keepers, who was out in the fields near Ottaiano, whilſt this combined ſtorm was at its height, was greatly ſurprized to find the drops of rain ſcald his face and hands, which phenomenon was probably occaſioned by the clouds having acquired a great degree of heat in paſſing through the above

above mentioned column of fire. The King of Naples did me the honour of informing me of this curious circumftance.

Sunday, Auguft the 8th, Vefuvius was quiet till towards fix o'clock in the evening, when a great fmoke began to gather again over its crater, and about an hour after, a rumbling fubteranneous noife was heard in the neighbourhood of the volcano; the ufual throws of red-hot ftones and *fcoriæ* began, and increafed every inftant. I was at this time at Paufilipo, in the company of feveral of my countrymen, obferving with good telefcopes the curious phenomena in the crater of Vefuvius, which, with fuch help, we could diftinguifh as well as if we had been actually feated on the fummit of the volcano. The crater feemed much enlarged by the violence of laft night's explofions, and the little mountain no longer exifted. At about nine o'clock there was a loud report, which fhook the houfes at Portici and its neighbourhood to fuch a degree as to alarm their inhabitants, and drive them out into the ftreets; and, as I have fince feen, many windows were broken, and walls cracked, by the concuffion of the air from that explofion, though faintly heard at Naples.

In an inftant a fountain of liquid tranfparent fire began to rife, and, gradually increafing, arrived at fo amazing a height

a height as to ftrike every one who beheld it with the moft awful aftonifhment. I fhall fcarcely be credited when I affure you, SIR, that, to the beft of my judgment, the height of this ftupendous column of fire could not be lefs than three times that of Vefuvius itfelf, which, as you know, rifes perpendicularly near 3700 feet above the level of the fea [(b)].

Puffs of fmoke, as black as can poffibly be imagined, fucceeded one another haftily, and accompanied the red-hot, tranfparent, and liquid lava, interrupting its fplendid brightnefs here and there by patches of the darkeft hue. Within thefe puffs of fmoke, at the very moment of their emiffion from the crater, I could perceive a bright, but pale, electrical fire, briskly playing about in zig zag lines [(i)].

The wind was S.W.; and though gentle was fufficient to carry thefe detached clouds or puffs of fmoke out of the column of fire, and a collection of them, by degrees, formed a black and extenfive curtain (if I may be al-

(*b*) Se tu fe' or lettore, a creder lento
Ciò, ch'e Io dirò, non farà maraviglia;
Che Io, che l' vidi; appena il mi confento.

DANTE INF. Cant. xxv. verfo 46.

(*i*) I mention this circumftance to prove, that the electrical matter, fo manifeft during this eruption, actually proceeded from the bowels of the volcano, and was not attracted from a great height in the air, and conducted into its crater by the vaft column of fmoke.

lowed

lowed the expreffion) behind it; in other parts of the fky it was perfectly clear, and the ftars were bright.

The fiery fountain, of fo gigantic a fize, upon the dark ground above mentioned, made the moft glorious contraft imaginable, and the blaze of it reflected ftrongly on the furface of the fea, which was at that time perfectly fmooth; added greatly to this fublime view.

The liquid lava, mixed with ftones and *fcoriæ*, after having mounted, I verily believe, at the leaft ten thoufand feet, was partly directed by the wind towards Ottaiano, and partly falling almoft perpendicularly, ftill red-hot and liquid, on Vefuvius, covered its whole cone, part of that of the mountain of Somma, and the valley between them. The falling matter being nearly as vivid and inflamed as that which was continually iffuing frefh from the crater, formed with it one complete body of fire, which could not be lefs than two miles and a half in breadth, and of the extraordinary height above mentioned, cafting a heat to the diftance of at leaft fix miles around it.

The brufh wood on the mountain of Somma was foon in a blaze, which flame, being of a different tint from the deep red of the matter thrown out of the volcano, and from the filvery blue of the electrical fire, ftill added to the contraft of this moft extraordinary fcene.

 The

The black cloud increasing greatly once bent towards Naples, and seemed to threaten this fair city with speedy destruction; for it was charged with electrical matter, which kept constantly darting about it in strong and bright zig zags, just like those described by PLINY the younger in his letter to TACITUS, and which accompanied the great eruption of Vesuvius that proved fatal to his uncle [(k)]. This volcanic lightning, however, as I particularly remarked, very rarely quitted the cloud, but usually returned to the great column of fire towards the crater of the volcano from whence it originally came [(l)]. Once or twice, indeed, I saw this lightning (or *ferilli* as it is called here) fall on the top of Somma, and set fire to some dry grass and bushes [(m)].

Fortunately

(k) " Ab altero latere, nubes atra, et horrenda, ignei spiritus tortis vi-" bratisque discursibus rupta, in longas flammarum figuras dehiscebat; fulgoribus " illæ, et similes et majores." PLIN. Epist.

(l) SORRENTINO mentions the like observation, which he made during an eruption of Vesuvius in 1707, when the same kind of black cloud bent over Naples; these are his words. " Alle ore 19. tutti i cittadini nelle oscure tenebre " si trovarono in mezzo delle Saëtte, delle quali, alcune vedeansi uscir dalla for-" nace del Vesuvio, e scorrere fino al capo di Pausilipo, d'onde non passando " più inanzi fuor la nuvola delle ceneri, o divertirsi altronde, indietro per " l'istessa linea tornarono a scopiar su la fornace, onde uscirono : qual moto re-" trogrado mai hopotuto intendere."

(m) Some time after the eruption had ceased, the air continued greatly impregnated with electrical matter. The Duke of Cotrofiano, a Neapolitan nobleman (who, from his superior knowledge in experimental philosophy and

 mechanics,

Fortunately for us the wind increasing from the S.W. quarter, carried back the threatning cloud just as it had reached the city, and began to occasion great alarm. All public diversions ceased in an instant, and the theatres being shut, the doors of the churches were thrown open. Numerous processions were formed in the streets, and women and children, with dishevelled heads, filled the air with their cries, insisting loudly upon the relics of St. Januarius being immediately opposed to the fury of the mountain: in short, the populace of this great city began to display its usual extravagant mixture of riot and bigotry, and if some speedy and well-timed precautions had not been taken, Naples would, perhaps, have been in more danger of suffering from the irregularities of its lower class of inhabitants than from the angry volcano.

But to return to my subject: after the column of fire had continued in full force near half an hour, the eruption ceased all at once, and Vesuvius remained sullen and silent. After the dazzling light of the fiery fountain [(n)],

mechanics, does honour to his country) told me, that having, about half an hour after the great eruption had ceased, held a Leyden bottle, armed with a pointed wire, out of his window at Naples, it soon became considerably charged. Whilst the eruption was in force, its appearance was too alarming to allow one to think of such experiments.

(n) The light diffused by this huge column of fire was so strong, that the most minute objects could be discerned clearly within the compass of ten miles or more round the mountain. Mr. MORRIS, an English gentleman, told me, that at Sorrento, which is twelve miles from Vesuvius, he read the title page of a book by that volcanic light.

all feemed dark and difmal, except the cone of Vefuvius, which was covered with glowing cinders and *fcoriæ*, from under which, at times, here and there, fmall ftreams of liquid lava efcaped, and rolled down the fteep fides of the volcano. This fcene put me in mind of MARTIAL's defcription of Etna:

Cuncta jacent flammis, et trifti merfa favillâ.

In the parts of Naples neareft Vefuvius, whilft the eruption lafted, a mixed fmell, like that of fulphur, with the vapours of an iron foundery, was fenfible; but nearer to the mountain that fmell was very offenfive, as I have often found it in my vifits to Vefuvius during an eruption.

Thus, SIR, have I endeavoured to convey to you at leaft a faint idea of a fcene fo glorious and fublime as, perhaps, may have never before been viewed by human eyes, at leaft in fuch perfection.

I am fenfible, from the traces of them I have obferved in the volcanic ftrata, which compofe the greateft part of this country, that there have been many more confiderable eruptions than the one juft defcribed; yet, moft probably, thofe very violent eruptions muft either have been attended with earthquakes, and other fuch alarming circumftances, as to make the beholders lefs attentive to the beauty of the fcenes fuch phenomena offered than to their

their own fafety; or clouds of fmoke and afhes, as is ufually the cafe in all great eruptions, muft have fo far obfcured the volcano, as to exhibit only a confufed mafs of fire and fmoke.

Whilft we had been enjoying the extraordinary fight of this gigantic fountain of liquid fire in perfect fafety, the unfortunate inhabitants of the other fide of the mountain of Somma, particularly at Ottaiano and Caccia-bella, were involved in that dark and footy cloud which formed fo proper a back ground to our bright picture, and were pelted with ftones and *fcoriæ* of lava; but I fhall prefently give you a particular defcription of their truly diftrefsful fituations, juft as I had it from many of the poor fufferers themfelves, when I vifited that part of the country a few days after this eruption.

Monday, Auguft the 9th, about nine o'clock in the morning, the fourth fever-fit of the mountain began to manifeft itfelf by the ufual fymptoms, fuch as a fubterraneous boiling noife, violent explofions of inflamed matter from the crater of the volcano, accompanied with fmoke and afhes, which fymptoms increafed every inftant. The fmoke was of two forts; the one as white as fnow, and the other as black as jet.

The white, as defcribed in the former part of this journal, rolled gently mafs over mafs, refembling bales

of

of the softest cotton; and the black composed of *scoriæ* and minute ashes shot up with force in the midst of the white smoke, which, from the minerals, was also sometimes tinged with yellow, blue, and green. Presently such a tremenduous mass of these accumulated clouds stood over Vesuvius as seemed to threaten Naples again, and actually made the mountain itself appear a mole-hill.

This day's eruption was similar to that of Thursday last, but many degrees more violent. Some stones, thrown near as high as those of last night, fell on the mountain of Somma, and set fire to the brush wood with which it is covered; but there being little wind, and that Westerly, the volcanic matter rose and fell in a more perpendicular direction, and Ottaiano did not suffer by this day's eruption; but most of the inhabitants of the towns, on the borders of Vesuvius, fled to Naples, alarmed by the tremendous clouds and the loud explosions.

We remarked, that several very large stones, after having mounted to an immense height, formed a parabola, leaving behind them a trace of white smoke that marked their course; some burst in the air exactly like bombs, and others fell into the valley between Somma and Vesuvius without bursting; others again burst into a thou-

a thoufand pieces foon after their emiffion from the crater: they might very properly be called volcanic bombs.

In the fmoke iffuing from the crater of Vefuvius we often remarked a fudden brifk and quivering motion, which feemed to communicate itfelf inftantaneoufly from one cloud to another, and fometimes affected thofe that were very high in the great mafs above the volcano. Though I could not difcern any electrical fire, yet I make no doubt, but that the effect above mentioned was occafioned by it, and would have been vifible in the night-time.

Upon the whole, this day's eruption was very alarming; until the lava broke out about two o'clock, and ran three miles between the two mountains, we were in continual apprehenfion of fome fatal event. It continued to run about three hours, during which time every other fymptom of the mountain fever gradually abated, and at feven o'clock at night all was calm.

It was univerfally remarked, that the air this night, for many hours after the eruption, was filled with meteors, fuch as are vulgarly called falling ftars; they fhot generally in an horizontal direction, leaving a luminous trace behind them, but which quickly difappeared. The night was remarkably fine, ftar-light, and without a cloud. This kind of electrical fire feemed to be harm-

lefs,

lefs, and never to reach the ground; whereas that with which the black volcanic cloud of laft night was pregnant appeared mifchievous, like the lightning that attends a fevere thunder ftorm, as we fhould undoubtedly have experienced, had the eruption continued longer, and the cloud fpread over Naples. The fame kind of lightning proved fatal to feveral people, and did great damage within the fpace of many miles round Vefuvius during its great eruption of 1631, as is mentioned in one of my former letters on this fubject.

During this day's eruption the relics of St. Januarius were carried in proceffion, and expofed to the furious mountain from the bridge of the Maddalena, amidft a prodigious concourfe of people, who are at this moment well convinced, that to this ceremony alone Naples may attribute its happy efcape.

It was from their Sicilian Majefties palace at Paufilipo that I made my obfervations on this day's eruption, and in the prefence of their Majefties, who had been pleafed to fend for me in the morning, as foon as the volcano became turbulent.

Tuefday, Auguft the 10th, Vefuvius was quiet.

Wednefday, Auguft the 11th, about fix o'clock in the morning, the fifth and laft fever-fit of the mountain came on, and gradually increafed. About twelve o'clock

it

it was at its height[(o)], and very violent indeed, the explosions being louder than those that attended the former eruptions, we could not judge of the height of the vollies of stones and *scoriæ*, as some rainy clouds were blended with the volcanic ones, and hid the upper part of the cone and crater of Vesuvius from our view.

The same mountains of white cotton-like clouds, piled one over another, rose to such an extraordinary height, and formed such a colossal mass over Vesuvius, as cannot possibly be described, or scarcely imagined. It may have been from a scene of this kind, that the ancient poets took their ideas of the giants waging war with Jupiter.

About five o'clock in the evening the eruption ceased, some rain having fallen this day, which having been greatly impregnated with the corrosive salts of the volcano, did much damage to the vines in its neighbourhood.

Thursday and Friday, the 12th and 13th of August, Vesuvius continued to smoke considerably, and at times slight explosions were heard, like cannon at a great distance; but there have been no more throws from its cra-

(*o*) It has been remarked by the oldest people in the neighbourhood of Vesuvius, that in its eruptions the volcano is subject to a crisis at noon and midnight; and, indeed, from my own observation, I believe that remark to be well-founded.

ter, nor any ſtreams of lava from its flanks, ſince Wedneſday laſt.

On Saturday, Auguſt the 15th, I went, accompanied by Count LAMBERG, the Imperial Miniſter at this Court, to viſit Ottaiano and Caccia-bella, the diſtrict which had been moſt ſeverely treated by the heavy and deſtructive ſhower of volcanic matter from the crater of Veſuvius laſt Sunday night.

Soon after having paſſed the town of Somma, we began to perceive, that the heat of the fiery ſhower, which had fallen in its neighbourhood, had affected the leaves of the trees and vines, which we found ſtill more parched and ſhrivelled in proportion as we approached the town of Ottaiano, which may be about three miles from Somma. At about the diſtance of a mile from Somma, we began to perceive freſh cinders or *ſcoriæ* of lava, thinly ſcattered on the road and in the fields. Every ſtep we advanced we found them of a larger dimenſion, and in greater abundance. At the diſtance of a mile and a half from Ottaiano, the ſoil was totally covered by them, and the leaves and fruit were either intirely ſtripped from the trees, or remained thinly on them, ſhrivelled and dried up by the intenſe heat of the volcanic ſhower.

After having paffed through the moft fertile country, abounding with trees loaded with fruits of every kind, and the moft luxuriant vegetation, through gay villages crouded with chearful inhabitants, to come at once to fuch a fcene of defolation and mifery, affording to our view nothing but heaps of black cinders and afhes, blafted trees, ruined houfes, with a few of their fcattered inhabitants juft returned with ghaftly, difmayed countenances, to furvey the havock done to their tenements and habitations, and from which they themfelves had with much difficulty efcaped alive on Sunday laft, was fuch a melancholy fcene as can neither be defcribed or forgotten.

We found the roof of his Sicilian Majefty's fporting feat at Caccia-bella much damaged by the fall of large ftones and heavy *fcoriæ*, fome of which, after having been broken by their fall through the roof, ftill weighed upwards of thirty pounds. This place, in a direct line, cannot be lefs than four miles from the crater of Vefuvius.

The moft authentic accounts have been received of the fall of fmall volcanic ftones and cinders (fome of which weighed two ounces) at Benevento, Foggia, and Monte Mileto, upwards of thirty miles from Vefu-

vius (p); but what is moſt extraordinary (as there was but little wind during the eruption of the eighth of Auguſt) minute aſhes fell thick that very night upon the town of Manfredonia, which is at the diſtance of an hundred miles from Veſuvius (q).

Theſe facts ſeem to confirm the extreme ſuppoſed height of the column of fire that iſſued from the crater of Veſuvius laſt Sunday night, and are greatly in ſupport of what we find recorded in the hiſtory of Veſuvius with reſpect to the fall of its aſhes at an amazing diſtance, and in a ſhort ſpace of time, during its violent eruptions.

We proceeeded from Caccia-bella to Ottaiano, which is a mile nearer to Veſuvius, and is reckoned to contain twelve thouſand inhabitants. Nothing could be more diſmal than the ſight of this town, unroofed, half buried

(p) The prince of Monte Mileto told me, that his ſon, the Duke of Popoli, who was at Monte Mileto the 8th of Auguſt, had been alarmed by the ſhower of cinders that fell there, ſome of which he had ſent to Naples, weighing two ounces; and that ſtones of an ounce had fallen upon an eſtate of his ten miles farther off. Monte Mileto is about thirty miles from the volcano.

(q) The Abbé GALIANI, well known in the literary world, told me, that his ſiſter, a nun in a Convent at Manfredonia, had wrote to enquire after him, imagining that Naples muſt have been deſtroyed, when they, at ſo great a diſtance, had been ſo much alarmed by a ſhower of minute aſhes, which fell on that city at eleven o'clock at night, the 8th of Auguſt, as to open all the churches, and go to prayers. As the great eruption happened at nine o'clock at night, the aſhes muſt have travelled an hundred miles within the ſhort ſpace of two hours.

under

under black *fcoriæ* and afhes, all the windows towards the mountain broken, and fome of the houfes themfelves burnt, the ftreets choaked up with thefe afhes (in fome that were narrow, the *ftratum* was not lefs than four feet thick), and a few of the inhabitants juft returned were employed in clearing them away, and piling up the afhes in hillocks to get at their ruined houfes. Others were affembled in little groups, inquiring after their friends and neighbours, relating each other's woes, croffing themfelves, and lifting up their eyes to Heaven when they mentioned their miraculous efcapes. Some monks, who were in their convent during the whole of the horrid fhower, gave us the following particulars, which they related with folemnity and precifion.

The mountain of Somma, at the foot of which Ottaiano is fituated, hides Vefuvius from its fight, fo that till the eruption became confiderable it was not vifible to them. On Sunday night, when the noife increafed, and the fire began to appear above the mountain of Somma, many of the inhabitants of this town flew to the churches, and others were preparing to quit the town, when a fudden violent report was heard; foon after which they found themfelves involved in a thick cloud of fmoke and minute afhes: a horrid clafhing noife was heard in the air, and prefently fell a deluge of ftones and large

large *ſcoriæ*, ſome of which *ſcoriæ* were of the diameter of ſeven or eight feet, and muſt have weighed more than an hundred pounds before they were broken by their fall, as ſome of the fragments of them, which I picked up in the ſtreets, ſtill weighed upwards of ſixty pounds. When theſe large vitrified maſſes either ſtruck againſt one another in the air, or fell on the ground, they broke in many pieces, and covered a large ſpace around them with vivid ſparks of fire, which communicated their heat to every thing that was combuſtible (r). In an inſtant the town, and country about it, was on fire in many parts; for in the vineyards there were ſeveral ſtraw huts, which had been erected for the watchmen of the grapes, all of which were burnt. A great magazine of wood in the heart of the town was all in a blaze, and, had there been much wind, the flames muſt have ſpread univerſally, and all the inhabitants would have infallibly been burnt in their houſes, for it was impoſſible for them to ſtir out. Some who attempted it with pillows, tables, chairs, the tops of wine caſks, &c. on their heads, were either knocked down, or ſoon driven back to their cloſe quarters under arches, and in the cellars of their houſes. Many

(r) Theſe maſſes were formed of the liquid lava, the exterior parts of which had become black and porous by cooling in the long traverſe they had made through the air, whilſt the interior parts, leſs expoſed, retained an extreme heat, and were perfectly red.

were

were wounded, but only two persons have died of the wounds they received from this dreadful volcanic shower. To add to the horror of the scene, incessant volcanic lightning was whisking about the black cloud that surrounded them, and the sulphureous smell and heat would scarcely allow them to draw their breath.

In this miserable and alarming situation they remained about twenty-five minutes, when the volcanic storm ceased all at once, and the frightened inhabitants of Ottaiano, apprehending a fresh attack from the turbulent mountain, hastily quitted the country, after having deposited the sick and bed-ridden, at their own desire, in the churches.

Had the eruption lasted an hour longer, Ottaiano must have remained exactly in the state of Pompeia, which was buried under the ashes of Vesuvius just 1700 years ago, with most of its inhabitants, whose bones are to this day frequently found under arches and in the cellars of the houses of that ancient city.

We were told of many miracles that had been wrought by the images of saints at this place during the late disaster; but, as they are quite foreign to my purpose, I shall, as usual, pass them over in silence.

The palace of the Prince of Ottaiano is situated on an eminence above the town, and nearer the mountain, the

steps

ſteps leading up to it, being deeply covered with volcanic matter, reſembled the cone of Veſuvius, and the white marble ſtatues on the baluſtrade made a ſingular appearance peeping from under the black aſhes, which had entirely covered both the baluſtrade and their pedeſtals. The roof of the palace was totally deſtroyed, and the windows were broken; but the houſe itſelf, being ſtrongly built, had not ſuffered much.

We had an opportunity of ſeeing here exactly the quality of the dreadful ſhower, as the volcanic matter, which broke through the roof of the palace, and fell into the garrets on the balconies and in the courts, had not been removed. It was compoſed of the *ſcoriæ* of freſh lava much vitrified, great and ſmall, mixed with fragments of ancient ſolid lavas of different ſorts: many pieces were enveloped by the new lava, which formed a cruſt about them; and others were only ſlightly varniſhed by the freſh lava. Theſe kind of ſtones being very compact, and ſome weighing eight or ten pounds, muſt have fallen with greater force than the heavier *ſcoriæ*, which were very porous, and had the great ſurface above mentioned.

The palace of Ottaiano is built on a thick *ſtratum* of ancient lava, which ran from the mountain of Somma when in its active volcanic ſtate. Under this *ſtratum* we

were

were fhewn three grottoes, from which iffues a conftant extreme cold wind, and at times with impetuofity, and a noife like water dafhing upon rocks. They are fhut up with doors like cellars, and are made ufe of as fuch, as alfo to keep provifions frefh and to cool liquors. I had never feen thefe *ventaroli* before. In my letter to Dr. MATY, upon the nature of the foil round Naples, I have mentioned others of the fame kind that I had met with on Vefuvius, Etna, and in the Ifland of Ifchia (s).

We obferved, that the tract of country completely covered with a *ftratum* of the volcanic matter above mentioned was about two miles and a half broad, and as much in length, in which fpace the vines and fruit trees were totally ftripped of their leaves and fruit, and had the appearance of being quite burnt up; but, to my great furprize, having vifited that country again two days ago, I faw thofe very trees, which were apple, pear, peach, and apricot, in bloffom again, and fome with the fruit already formed, and of the fize of hazel-nuts. The vines there had alfo put forth frefh leaves, and were in

(s) At Cefi, in the Roman State, towards the Adriatic, there are many fuch *ventaroli*; and the inhabitants of that town, by means of leaden pipes, conduct the frefh air from them into the very rooms of their houfes, fo that by turning a cock they can cool them to any degree. Some who have refined ftill more upon this luxury, by fmaller pipes, bring this cold air under the dining-table, fo as to cool the bottle of liquor upon it.

 bloom.

bloom. Many foxes, hares, and other game, were deſtroyed by the fiery ſhower in the diſtrict of Somma and Ottaiano (t)

His Sicilian Majeſty, whoſe goodneſs of heart inclines him on all occaſions to ſhew his benevolence and aſſiſt the unfortunate, has ordered a conſiderable ſum of money to be diſtributed among the unhappy ſufferers of Ottaiano and its neighbourhood.

On the 18th of September I went upon Mount Veſuvius, accompanied by Lord HERBERT and my uſual guide. We could not poſſibly reach its crater, being covered with a thick ſmoke, too ſulphureous and offenſive to be encountered; neither would it have been prudent to have ventured up, had there not been that impediment, as it was evident, from the loud reports we heard from time to time, that there exiſted ſtill a great fermentation within the bowels of the volcano. We therefore contented ourſelves with examining the effects of the late extraordinary eruption on its cone, and in the valley between it and the mountain of Somma.

The conical part of Veſuvius is now covered with fragments of lava and *ſcoriæ*, which makes the aſcent

(t) Having had the honour of being on a ſhooting party lately with the King of Naples, at the foot of Veſuvius and Somma, ſeveral dead hares were found, and we killed others whoſe backs were quite bare, the fur having been ſinged off of them by the hot aſhes.

much

much more difficult and troubleſome than when it was only covered with minute aſhes. The particularity of this laſt eruption was, that the lava which uſually ran out of the flanks of the volcano, forming caſcades, rivers, and rivulets of liquid fire, was now chiefly thrown up from its crater in the form of a gigantic fountain of fire (u), which falling ſtill in ſome degree of fuſion has, in a manner,

(*u*) SORRENTINO mentions, in his Iſtoria del Veſuvio, that the volcano in 1676 vented itſelf in the like manner. "Non a torrenti modo mandò fuori le "ſue viſcere, ma tutti in aria menòlla." Such wonderful, violent, and ſudden emiſſions of liquid lava muſt have been occaſioned by ſome accidental and extraordinary cauſe; and I was inclined to think, that a ſudden communication of water with the lava in fuſion might be the occaſion of ſuch a phenomenon, particularly as we know that pools of rain-water have been found formerly in caverns within the bowels of Veſuvius; and that a river, ſuppoſed to be that anciently called Draco, and which was buried by an ancient eruption, burſt out ſome years ago with ſuch force, from under a *ſtratum* of lava at Torre del Greco, as to be ſufficient to turn mills there; but a late curious experiment, mentioned by Monſ. DE FAUJAS, in his Recherches ſur les Volcans éteints, p. 176. ſeems to contradict my ſuppoſition; and that water introduced to the furnace of a volcano, finding there a more rarified air, would not produce an exploſion. Monſ. DESLAUDES, Director of the Royal Manufacture of Looking-glaſs at St. Gobin, made the following experiment in 1768, in the preſence of the Duke DE LA ROCHEFOUCAULT, Monſ. DE FAUJAS, and others. He poured ſome water upon a quantity of glaſs in fuſion, and which had been in that ſtate in the crucible for twelve hours. The water did not occaſion the leaſt fermentation; but, on the contrary, rolled upon its ſurface, without even producing any ſmoke, and after having become ſeemingly red-hot, like the metal in fuſion, diſappeared in about three minutes, without having occaſioned the leaſt exploſion. If the great emiſſions of lava above mentioned were not then occaſioned by water mixing with the lava, may not they have been produced by violent ſubter-

a manner, cafed up the conical part of Vefuvius with a *ftratum* of hard *fcoriæ:* on the fide next the mountain of Somma, that *ftratum* is furely more than one hundred feet thick, forming a high ridge. The valley between Vefuvius and Somma has received fuch a prodigious quantity of lava and other volcanic matter during this laft eruption, that it is raifed, as is imagined, two hundred and fifty feet or more. Three fuch eruptions as the laft would completely fill up the valley, and, by uniting Vefuvius and Somma, form them into one mountain, as they moft probably were before the great eruption in the reign of TITUS. In fhort, I found the whole face of Vefuvius changed. Thofe curious channels, in which the lava ran in the month of May laft, are all buried. The volcano appears to have likewife increafed in height; the form of the crater is changed, a great piece of its rim towards Somma being wanting; and on the fide towards the fea it is alfo broken. There are fome very large cracks towards the point of the cone of the volcano, which makes it probable, that more of the borders of the crater will fall in. The ridge of frefh volcanic matter on the cone of Vefuvius towards Somma, and the thick *ftratum* in the valley, are likewife

fubterraneous exhalations having forced their way into the cauldron of the volcano (if I may be allowed the expreffion) replete with matter in fufion, and blown its whole contents, with what even oppofed its paffage, at once into the air?

full

full of cracks, from which there iffues a conftant fulphureous fmoke that tinges them and the circumjacent *fcoriæ* and cinders with a deep yellow, or fometimes a white tint. Thefe laft mentioned cracks, though deep, do not, as I apprehend, pafs the *ftratum* formed by the laft eruption, and which, from its extreme thicknefs, particularly in the valley, will probably retain a great degree of heat for fome years to come, as did a thick *ftratum* of lava that ran into the *foffa grande* in the year 1767.

The number and fize of the ftones, or, more properly fpeaking, of the fragments of lava which have been thrown out of the volcano in the courfe of the laft eruption, and which lie fcattered thick on the cone of Vefuvius, and at the foot of it, is really incredible. The largeft we meafured was in circumference no lefs than one hundred and eight Englifh feet, and feventeen feet high. It is a folid block, and is much vitrified: in fome parts of it there are large pieces of pure glafs, of a brown yellow colour, like that of which our common bottles are made, and throughout its pores feem to be filled with perfect vitrifications of the fame fort. The fpot where it alighted is plainly marked by a deep impreffion almoft at the foot of the cone of the volcano, and it took three bounds before it fettled, as is plainly perceived by the marks

marks it has left on the ground, and by the ſtones which it has pounded to atoms under its prodigious weight. When we conſider the enormous ſize and weight of ſuch a ſolid maſs, thrown at leaſt a quarter of a mile clear of the mouth of the volcano, we can but admire the wonderful powers of nature, of which, being ſo very ſeldom within the reach of human inſpection, we are in general too apt to judge upon much too ſmall a ſcale.

Another ſolid block of ancient lava, ſixty-ſix feet in circumference, and nineteen feet high, being nearly of a ſpherical ſhape, was thrown out at the ſame time, and lies near the former. This ſtone, which has the marks of having been rounded, nay almoſt poliſhed, by continual rolling in torrents, or on the ſea-ſhore, and which yet has been ſo undoubtedly thrown out of the volcano, may be the ſubject of curious ſpeculations [x]. Another block of ſolid lava that was thrown much farther, and lies in the valley between the cone of Veſuvius and the Hermitage, is ſixteen feet high, and ninety-two feet in circumference, though it plainly appears, by the large fragments that lie round, and were detached from it by the ſhock

(x) Or may not this ſtone be a ſpherical volcanic baſalt, ſuch as one of forty-five feet in circumference, deſcribed by Monſ. FAUJAS DE ST. FOND, in p. 155. of his curious book on the ſubject of extinguiſhed volcanos?

of

of its fall, that it muft have been twice as confiderable when in the air.

There are thoufands of very large fragments of different fpecies of ancient and modern lavas, that lie fcattered by the late explofions on the cone of Vefuvius, and in the vallies at its foot; but thefe three were the largeft of thofe we meafured [(y)].

We found alfo many fragments of thofe volcanic bombs that burft in the air, as mentioned in the former part of this journal; and fome entire, having fallen to the ground without burfting. The frefh red-hot and liquid lava having been thrown up with numberlefs fragments of ancient lavas, the latter were often clofely enveloped by the former; and probably when fuch fragments of lava were porous and full of air bubbles, as is often the cafe, the extreme outward heat, fuddenly rarifying the confined air, caufed an explofion. When thefe fragments were of a more compact lava they did not explode, but were fimply inclofed by the frefh lava, and acquired a fpherical form by whirling in the air, or rolling down the fteep fides of the volcano.

(y) We meafured two other ftones in the valley between Somma and Vefuvius; the one was twenty-two feet and a half long, thirteen feet and a half broad, and ten feet high; the other, eleven feet and a half high, and feventy-two feet in circumference.

The ſhell or outward coat of the bombs that burſt, and of which we found ſeveral pieces, was always compoſed of freſh lava, in which many ſplinters of the more ancient lava that had been incloſed are ſeen ſticking. I was much pleaſed with this diſcovery, having been greatly puzzled for an explanation of this volcanic operation, which was new to me, and which was very frequent during the eruption of the 9th of Auguſt.

The phenomenon of the natural ſpun-glaſs, which fell at Ottaiano with the aſhes on the 5th of Auguſt, was likewiſe clearly explained to me here. I have already mentioned, that the lava thrown up by this eruption was in general more perfectly vitrified than that of any former eruption, which appeared plainly upon a nearer examination of the fragments of freſh lava, the pores of which we generally found full of a pure vitrification, and the *ſcoriæ* themſelves, upon a cloſe examination with a magnifying glaſs, appeared like a confuſed heap of filaments of a foul vitrification. When a piece of the ſolid freſh lava had been cracked in its fall without ſeparating entirely, we always ſaw capillary fibres of perfect glaſs, reaching from ſide to ſide within the cracks. If I may be allowed a mean compariſon, which, however, conveys the idea of what I wiſh to explain better than any other I can think of, this lava reſembled a rich Parmeſan

cheeſe,

cheefe, which, when broken and gently feparated, fpins out tranfparent filaments from the little cells that contained the clammy liquor of which thofe filaments were compofed. The natural fpun-glafs, then, that fell at Ottaiano during this eruption, as well as that which fell in the Ifle of Bourbon in the year 1766, muft have been formed, moft probably, by the operation of fuch a fort of lava as has been juft defcribed, cracking and feparating in the air at the time of its emiffion from the craters of the volcanos, and by that means fpinning out the pure vitrified matter from its pores or cells, the wind at the fame time carrying off thofe filaments of glafs as faft as they were produced.

I obferved, fticking to fome very large fragments of the new lava, which were of a clofe grain, fome pieces of a fubftance, whofe texture very much refembled that of a true pumice ftone; and, upon a clofe examination, and having feparated them from the lava, I perceived, that this fubftance had actually been forced out of the minute pores of the folid ftone itfelf, and was a collection of fine vitreous fibres or filaments, confounded together at the time of their being preffed out by the contraction of the large fragments of lava in cooling, and which had bent downwards by their own weight. This curious fubftance has the lightnefs of a

pumice, and resembles it in every respect except being of a darker colour.

When the pores of the fresh solid lava were large and filled with pure vitrified matter, we found that matter sometimes blown into bubbles on its surface, I suppose, by the air which had been forced out at the time the lava contracted itself in cooling: those bubbles, being thin, shewed that this volcanic glass has the kind of transparency of our common glass bottles, and is like them of a dirty yellow colour. I detached with a hammer some large pieces of this kind of glass as big as my fist, which adhered to, and was incorporated with, some of the larger fragments of lava, and, though of the same kind, from their thickness they appeared perfectly black, and were opaque.

Another particularity is remarkable in the lava of this eruption: many detached pieces of it are in the shape of a barley-corn or of a plumb-stone, small at each end, and thick in the middle. We picked up several, and saw many more which were too heavy for us to carry off, for they must have weighed more than sixty pounds; some of the smaller ones did not weigh an ounce. I suppose them to be drops from the liquid fountain of fire of the 8th of August, which might very naturally acquire such a form in their fall; but the peasants in the neigh-
bourhood

bourhood of Vefuvius are well convinced that they are the thunder-bolts that fell with the volcanic lightning.

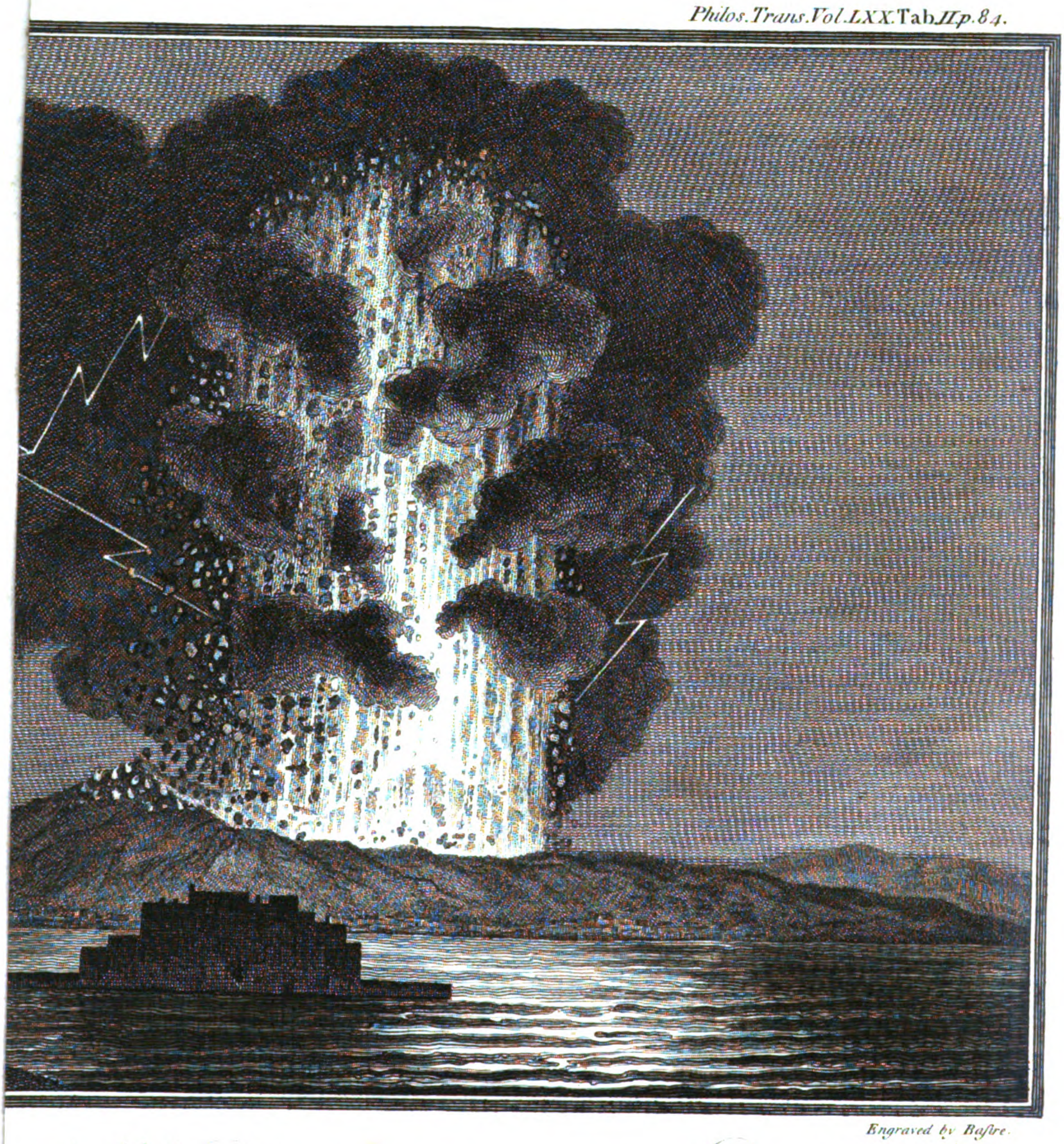
We found many of the volcanic bombs or, properly fpeaking, round balls of frefh lava, large and fmall; all of which have a *nucleus* compofed of a fragment of more ancient and folid lava. There were alfo fome other curious vitrifications, very different from any I had ever feen before, mixed with the late fallen fhower of huge *fcoriæ* and maffes of lava.

Though I have endeavoured to be as particular and clear as poffible in the defcription I have given of the curious fubftances produced by the late eruption of Vefuvius, yet, as fpecimens of thofe fubftances will explain more at one fight than I can pretend to do by whole pages in writing, I fhall not fail to fend you, by the firft favourable opportunity, a collection of them, which I have fet apart for that purpofe, particularly as, I flatter myfelf, they may ferve to give fome light into a hitherto obfcure fubject: I mean, the nature and manner of the formation of pumice-ftones.

Vefuvius continues to fmoke confiderably, and we had a flight fhock of an earthquake yefterday; fo that I do not think, notwithftanding the late eruptions having been fo very confiderable, that the volcano has vented itfelf fo fufficiently as to remain long quiet.

I muſt now, SIR, beg your pardon if I have treſpaſſed too much upon your time: I meant to be ſhort, clear, and explicit; and if, by aiming at the two latter, I have failed in the former, I hope I ſhall be excuſed, and that you will pleaſe to take the will for the deed.

I am, &c.



Engraved by Basire.

of M. Vesuvius Augt. 8th 1779. from Pausilipo.

V. *An Appendix to the Paper in the Philosophical Transactions for the Year* 1778, *Number* XLII, *pages* 902 *et seq. intitled*, "*A Method of extending* Cardan's "*Rule for resolving one Case of the Cubick Equation* "$x^3 - qx = r$ *to the other Case of the same Equation*, "*which it is not naturally fitted to solve, and which is* "*therefore called* the irreducible Case." *By* Francis Maseres, *Esq. F. R. S. Cursitor Baron of the Exchequer.*

Read Nov. 4, 1779.

ARTICLE I.

IN the above-mentioned paper in the Philosophical Transactions the expression $\sqrt{^3}e \times$ the infinite series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. is shewn to be equal to the root of the equation $x^3 - qx = r$, whenever $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, but greater than one half of it, or than $\frac{q^3}{54}$. This expression is wholly transcendental, or composed of an infinite number of terms, to wit, the terms of the series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. multiplied into the cube-root of e. But I have since thought that it might be convenient on some occasions to divide this expression, if possible, into two others, whereof the one should be a

mere algebraick expreſſion, or conſiſt of a finite number of terms, and the other ſhould be tranſcendental, or involve in it an infinite ſeries. And I have accordingly diſcovered a method of doing this, which I will now proceed to deſcribe.

ART. 2. In the above-mentioned paper in the Philoſophical Tranſactions I denoted the exceſs of $\frac{q^3}{27}$ above $\frac{rr}{4}$ in the ſecond caſe of the equation $x^3 - qx = r$, as well as the exceſs of $\frac{rr}{4}$ above $\frac{q^3}{27}$ in the firſt caſe of it, by the letters ss. But I have ſince thought that it might have been better to denote the exceſs of $\frac{q^3}{27}$ above $\frac{rr}{4}$ in the ſecond caſe of that equation by the letters zz, in order the more clearly to diſtinguiſh it from the oppoſite difference $\frac{rr}{4} - \frac{q^3}{27}$ in the firſt caſe of it, which was denoted by ss. And I therefore in the courſe of the following pages ſhall uſe the letters zz inſtead of ss to denote the ſaid exceſs of $\frac{q^3}{27}$ above $\frac{rr}{4}$ in the ſecond caſe of the ſaid equation, or the difference $\frac{q^3}{27} - \frac{rr}{4}$.

ART. 3. Now, if zz be ſubſtituted inſtead of ss in the expreſſion $\sqrt{}^3 e \times$ the infinite ſeries $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. that expreſſion will thereby be converted into the following expreſſion, to wit, $\sqrt{}^3 e \times$ the infinite ſeries $2 + \frac{2zz}{9ee} - \frac{20z^4}{243e^4} + \frac{308z^6}{6561e^6} -$ &c. Therefore, if $\frac{rr}{4}$ be leſs

less than $\frac{q^3}{27}$ but greater than $\frac{q^3}{54}$, and e be put $= \frac{r}{2}$, and zz be put $= \frac{q^3}{27} - \frac{rr}{4}$, the root of the equation $x^3 - qx = r$ will be equal to $\sqrt{^3}e \times$ the infinite series $2 + \frac{2zz}{9ee} - \frac{20z^4}{243e^4} + \frac{308z^6}{6561e^6} -$ &c.

ART. 4. The numeral coefficients $\frac{2}{9}$, $\frac{20}{243}$, $\frac{308}{6561}$, &c. of $\frac{zz}{ee}$, $\frac{z^4}{e^4}$, $\frac{z^6}{e^6}$, &c. in this series are exactly double of $\frac{1}{9}$, $\frac{10}{243}$, $\frac{154}{6561}$, &c. which are the numeral coefficients of the same powers of the fraction $\frac{z}{e}$ in the series $1 + \frac{z}{3e} - \frac{zz}{9ee} + \frac{5z^3}{81e^3} - \frac{10z^4}{243e^4} + \frac{22z^5}{729e^5} - \frac{154z^6}{6561e^6} + \frac{2618z^7}{137,781e^7} -$ &c. which is equal to the cube-root of the binomial quantity $1 + \frac{z}{e}$; or, if the numeral coefficients of the said latter series be denoted by the capital letters A, B, C, D, E, F, G, H, &c. respectively, so that A shall be $= 1$, $B = \frac{1}{3}$, $C = \frac{1}{9}$, $D = \frac{5}{81}$, $E = \frac{10}{243}$, $F = \frac{22}{729}$, $G = \frac{154}{6561}$, and $H = \frac{2618}{137,781}$, and so on, the said numeral coefficients $\frac{2}{9}$, $\frac{20}{243}$, $\frac{308}{6561}$, &c. will be equal to 2C, 2E, 2G, &c. and the series mentioned in the last Article will be $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} -$ &c. and consequently the root of the equation $x^3 - qx = r$, in the second case of it, in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, will be equal to the expression $\sqrt{^3}e \times$ the series $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} -$ &c.

ART. 5. Now the series $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} -$ &c.

is

is equal to the ſum of the two following ſerieſes, to wit, $2A - \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} - \frac{2Gz^6}{e^6} -$ &c. (in which all the terms following the firſt term are marked with the ſign $-$, or are ſubtracted from the firſt term), and $\frac{4Czz}{ee} + \frac{4Gz^6}{e^6} +$ &c; and the ſeries $2A - \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} - \frac{2Gz^6}{e^6} -$ &c. is equal to the ſum of the two ſerieſes $A + \frac{Bz}{e} - \frac{Czz}{ee} + \frac{Dz^3}{e^3} - \frac{Ez^4}{e^4} + \frac{Fz^5}{e^5} - \frac{Gz^6}{e^6} +$ &c. and $A - \frac{Bz}{e} - \frac{Czz}{ee} - \frac{Dz^3}{e^3} - \frac{Ez^4}{e^4} - \frac{Fz^5}{e^5} - \frac{Gz^6}{e^6} -$ &c. which are reſpectively equal to the cube-roots of the binomial quantities $1 + \frac{z}{e}$ and $1 - \frac{z}{e}$. Therefore the ſeries $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} -$ &c. is $= \sqrt[3]{1 + \frac{z}{e}} + \sqrt[3]{1 - \frac{z}{e}} +$ the infinite ſeries $\frac{4Czz}{ee} + \frac{4Gz^6}{e^6} +$ &c. Conſequently the expreſſion $\sqrt[3]{e} \times$ the ſeries $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} -$ &c. is $= \sqrt[3]{e} \times \sqrt[3]{1 + \frac{z}{e}} + \sqrt[3]{e} \times \sqrt[3]{1 - \frac{z}{e}} + \sqrt[3]{e} \times$ the infinite ſeries $\frac{4Czz}{ee} + \frac{4Gz^6}{e^6} +$ &c. $= \sqrt[3]{e+z} + \sqrt[3]{e-z} + \sqrt[3]{e} \times$ the infinite ſeries $\frac{4Czz}{ee} + \frac{4Gz^6}{e^6} +$ &c. $= \sqrt[3]{e+z} + \sqrt[3]{e-z} + 4\sqrt[3]{e} \times$ the ſeries $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} + \frac{Tz^{18}}{e^{18}} +$ &c. Therefore the root of the equation $x^3 - qx = r$, in the ſecond caſe of it, in which $\frac{rr}{4}$ is leſs than $\frac{q^3}{27}$, is equal to $\sqrt[3]{e+z} + \sqrt[3]{e-z} + 4\sqrt[3]{e} \times$ the ſeries $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} + \frac{Tz^{14}}{e^{14}} +$ &c.; of which expreſſion the firſt part, to

wit,

wit, $\sqrt[3]{e+z} + \sqrt[3]{e-z}$ is algebraick, and the latter part, to wit, $\sqrt[3]{e} \times$ the series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} + \frac{Tz^{18}}{e^{18}} +$ &c. is transcendental. Q. E. I.[a]

Of the convergency of the Series obtained in the preceding Article.

ART. 6. This series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} + \frac{Tz^{18}}{e^{18}} +$ &c. evidently converges faster than the series $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} -$ &c. or $2 + \frac{2zz}{9ee} - \frac{20z^4}{243e^4} + \frac{308z^6}{6561e^6} -$ &c: and consequently the expression $\sqrt[3]{e+z} + \sqrt[3]{e-z} + 4\sqrt[3]{e} \times$ the series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} + \frac{Tz^{18}}{e^{18}} +$ &c. seems rather fitter

(a) N. B. I have been informed that both this mixed expression of the root of the equation $x^3 - qx = r$ in the second case of it, and the merely transcendental expression of it published in the former paper, and from which this expression is derived, were invented by Monsieur NICOLE, and published in the memoirs of the French Academy of Sciences so long ago as the year 1738; and the latter of them, to wit, the transcendental expression $\sqrt[3]{e} \times$ the series $2 + \frac{2ss}{9ee} - \frac{20s^4}{243e^4} + \frac{308s^6}{6561e^6} -$ &c. I had myself seen many years ago in Monsieur CLAIRAUT's algebra, in the place cited in the 50th Article of my former paper, to wit, in pages 286, 287, 288. But it was obtained by the intervention of negative quantities, and the roots of negative quantities, which gave it, in my opinion, an air of great obscurity. And therefore I thought an investigation of the same series, by a method that keeps clear of those difficulties, might not be unacceptable to the lovers of these sciences, nor unworthy of a place in the Transactions of this learned body.

 to

to exhibit the value of x in the equation $x^3 - qx = r$, in the second case of it (in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$), to a considerable degree of exactness than the former expression $\sqrt{3e} \times$ the series $2 + \frac{2zz}{9ee} - \frac{20z^4}{243e^4} + \frac{308z^6}{6561e^6} -$ &c.

A Computation of the four first Terms of the Series obtained in Art. 5.

ART. 7. The first fifteen terms of the infinite series which is equal to the cube-root of $1 + \frac{z}{e}$ are as follows; to wit, $1 + \frac{z}{3e} - \frac{zz}{9ee} + \frac{5z^3}{81e^3} - \frac{10z^4}{243e^4} + \frac{22z^5}{729e^5} - \frac{154z^6}{6561e^6} + \frac{2618z^7}{137{,}781e^7} - \frac{935z^8}{59{,}049e^8} + \frac{21505z^9}{1{,}594{,}323e^9} - \frac{55913z^{10}}{4{,}782{,}969e^{10}} + \frac{147{,}407z^{11}}{14{,}348{,}907e^{11}} - \frac{1{,}179{,}256z^{12}}{129{,}140{,}163e^{12}} + \frac{3{,}174{,}920z^{13}}{387{,}420{,}489e^{13}} - \frac{60{,}323{,}480z^{14}}{8{,}135{,}830{,}269e^{14}} +$ &c; or, in decimal fractions, $1 +$
$.333{,}333{,}333$, &c. $\times \frac{z}{e} - .111{,}111{,}111$, &c. $\times \frac{zz}{ee} +$
$.061{,}728{,}395$, &c. $\times \frac{z^3}{e^3} - .041{,}152{,}263$, &c. $\times \frac{z^4}{e^4} +$
$.030{,}178{,}326$, &c. $\times \frac{z^5}{e^5} - .023{,}472{,}031$, &c. $\times \frac{z^6}{e^6} +$
$.019{,}001{,}167$, &c. $\times \frac{z^7}{e^7} - .015{,}834{,}305$, &c. $\times \frac{z^8}{e^8} +$
$.013{,}488{,}482$, &c. $\times \frac{z^9}{e^9} - .011{,}690{,}017$, &c. $\times \frac{z^{10}}{e^{10}} +$
$.010{,}273{,}045$, &c. $\times \frac{z^{11}}{e^{11}} - .009{,}131{,}595$, &c. $\times \frac{z^{12}}{e^{12}} +$
$.008{,}195{,}021$, &c. $\times \frac{z^{13}}{e^{13}} - .007{,}414{,}542$, &c. $\times \frac{z^{14}}{e^{14}}$.

Therefore the four first terms of the series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} +$

$\frac{Lz^{10}}{e^{10}}+\frac{Pz^{14}}{e^{14}}+\frac{Tz^{18}}{e^{18}}+$ &c. are $\frac{zz}{9ee}+\frac{154z^6}{6561e^6}+\frac{559134z^{10}}{4,782,969e^{10}}+\frac{60,323,480z^{14}}{8,135,830,269e^{14}}$, or, in decimal fractions, $.111,111,111$, &c. $\times\frac{zz}{ee}+$ $.023,472,031$, &c. $\times\frac{z^6}{e^6}+.011,690,017$, &c. $\times\frac{z^{10}}{e^{10}}+$ $.007,414,542$, &c. $\times\frac{z^{14}}{e^{14}}$. Therefore the root of the cubick equation $x^3-qx=r$, in the second case of it (in which $\frac{rr}{4}$ is less than $\frac{q^3}{27}$), is equal to $\sqrt[3]{e+z}+\sqrt[3]{e-z}+$ $4\sqrt[3]{e}\times$ the series $\frac{zz}{9ee}+\frac{154z^6}{6561e^6}+\frac{55,913z^{10}}{4,782,969e^{10}}+\frac{60,323,480z^{14}}{8,135,830,269e^{14}}+$ &c. *ad infinitum*, or $\sqrt[3]{e+z}+\sqrt[3]{e-z}+4\sqrt[3]{e}\times$ the series $.111,111,111$, &c. $\times\frac{zz}{ee}+.023,472,031$, &c. $\times\frac{z^6}{e^6}+$ $.011,690,017$, &c. $\times\frac{z^{10}}{e^{10}}+.007,414,542$, &c. $\times\frac{z^{14}}{e^{14}}+$ &c. *ad infinitum.*

Of the best Manner of Proceeding to the Computation of more Terms of the said Series, if required.

ART. 8. If more than four terms of this last series are required, it will be necessary to compute the series $1+\frac{z}{3e}-\frac{zz}{9ee}+\frac{5z^3}{81e^3}-\frac{10z^4}{243e^4}+\frac{22z^5}{729e^5}-\frac{154z^6}{6561e^6}+$ &c. (which is $=\sqrt[3]{1+\frac{z}{e}}$), to more than fifteen terms; in order to which it will be convenient to express the terms of that series in the following manner, to wit, $1+\frac{1}{3}\times\frac{Az}{e}-\frac{2}{6}\times\frac{Bzz}{ee}+\frac{5}{9}\times\frac{Cz^3}{e^3}-\frac{8}{12}\times$ $\frac{Dz^4}{e^4}+\frac{11}{15}\times\frac{Ez^5}{e^5}-\frac{14}{18}\times\frac{Fz^6}{e^6}+\frac{17}{21}\times\frac{Gz^7}{e^7}-\frac{20}{24}\times\frac{Hz^8}{e^8}+\frac{23}{27}\times\frac{Iz^9}{e^9}-\frac{26}{30}\times\frac{Kz^{10}}{e^{10}}+\frac{29}{33}\times$

$\frac{Lz^{11}}{e^{11}} - \frac{32}{36} \times \frac{Mz^{12}}{e^{12}} + \frac{35}{39} \times \frac{Nz^{13}}{e^{13}} - \frac{38}{42} \times \frac{Oz^{14}}{e^{14}} + \frac{41}{45} \times \frac{Pz^{15}}{e^{15}} - \frac{44}{48} \times \frac{Qz^{16}}{e^{16}} + \frac{47}{51} \times$ $\frac{Rz^{17}}{e^{17}} - \frac{50}{54} \times \frac{Sz^{18}}{e^{18}} + \frac{53}{57} \times \frac{Tz^{19}}{e^{19}} - \frac{56}{60} \times \frac{Vz^{20}}{e^{20}} + \frac{59}{63} \times \frac{Wz^{21}}{e^{21}} - \frac{62}{66} \times \frac{Xz^{22}}{e^{22}} +$ &c. or $A + \frac{Bz}{e} - \frac{Czz}{ee} + \frac{Dz^3}{e^3} - \frac{Ez^4}{e^4} + \frac{Fz^5}{e^5} - \frac{Gz^6}{e^6} + \frac{Hz^7}{e^7} - \frac{Iz^8}{e^8} + \frac{Kz^9}{e^9} - \frac{Lz^{10}}{e^{10}} + \frac{Mz^{11}}{e^{11}} - \frac{N^{12}}{e^{12}} +$ $\frac{Oz^{13}}{e^{13}} - \frac{Pz^{14}}{e^{14}} + \frac{41}{45} \times \frac{Pz^{15}}{e^{15}} - \frac{44}{48} \times \frac{Qz^{16}}{e^{16}} + \frac{47}{51} \times \frac{Rz^{17}}{e^{17}} - \frac{50}{54} \times \frac{Sz^{18}}{e^{18}} + \frac{53}{57} \times \frac{Tz^{19}}{e^{19}} - \frac{56}{60} \times$ $\frac{Vz^{20}}{e^{20}} + \frac{59}{63} \times \frac{Wz^{21}}{e^{21}} - \frac{62}{66} \times \frac{Xz^{22}}{e^{22}} +$ &c; in which it is evident that the generating fractions of the coefficients of the ſeveral terms are derived from thoſe that immediately precede them by the continual addition of the number 3 to both their numerators and denominators.

An example of the reſolution of a cubick equation by means of the expreſſion $\sqrt[3]{e+z} + \sqrt[3]{e-z} + 4\sqrt[3]{e} \times$ *the series* $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} + \frac{Tz^{18}}{e^{18}} +$ *&c. given in Art.* 5.

ART. 9. Let it be required to reſolve the equation $x^3 - x = \frac{1}{3}$ by means of the ſaid expreſſion.

Here q is $= 1$, and $r = \frac{1}{3}$; and conſequently q^3 is $= 1$, and $\frac{q^3}{27} = \frac{1}{27}$, and $\frac{r}{2} = \frac{1}{6}$, and $\frac{rr}{4} = \frac{1}{36}$, which is leſs than $\frac{1}{27}$, or $\frac{q^3}{27}$. Therefore this equation does not come under CARDAN's rule, but may be reſolved by the expreſſion given in Art. 5, provided that $\frac{rr}{4}$, though leſs than $\frac{q^3}{27}$, is greater than half $\frac{q^3}{27}$, or than $\frac{q^3}{54}$; which it is, becauſe

it is $=\frac{1}{36}$, whereas $\frac{q^3}{54}$ is equal only to $\frac{1}{54}$, which is less than $\frac{1}{36}$. Therefore the proposed equation $x^3-x=\frac{1}{3}$ may be resolved by means of the said expression.

ART. 10. Now, since in this case q is $=1$, and r is $=\frac{1}{3}$, we shall have $\frac{r}{2}$, or e, $=\frac{1}{6}$, and $\frac{q^3}{27}-\frac{rr}{4}\left(=\frac{1}{27}-\frac{1}{36}=\frac{4}{108}-\frac{3}{108}=\frac{1}{108}\right)=\frac{1}{36\times 3}$; that is, zz will be $=\frac{1}{36\times 3}$, and consequently z will be $=\frac{1}{6\sqrt{3}}$. Therefore $e+z$ will be $=\frac{1}{6}+\frac{1}{6\sqrt{3}}\left(=\frac{\sqrt{3}}{6\sqrt{3}}+\frac{1}{6\sqrt{3}}=\frac{3}{6\times 3}+\frac{\sqrt{3}}{6\times 3}=\frac{3+\sqrt{3}}{6\times 3}=\frac{3+\sqrt{3}}{18}=\frac{3+1.732,050,8}{18}=\frac{4.732,050,8}{18}\right)=.262,891,71$; and $e-z$ will be $=\frac{1}{6}-\frac{1}{6\sqrt{3}}\left(=\frac{3-\sqrt{3}}{18}=\frac{3-1.732,050,8}{18}=\frac{1.267,949,2}{18}\right)=.070,441,62$. Therefore the cube-root of $e+z$ is $=\sqrt[3]{.262,891,71}=.640,607,91$; and the cube-root of $e-z$ is $=\sqrt[3]{.070,441,62}=.412,993,40$; and consequently $\sqrt[3]{e+z}+\sqrt[3]{e-z}$ is $=.640,607,91+.412,993,40=1.053,601,31$.

ART. 11. It remains that we compute the infinite series $\frac{Czz}{ee}+\frac{Gz^6}{e^6}+\frac{Lz^{10}}{e^{10}}+\frac{Pz^{14}}{e^{14}}+\frac{Tz^{16}}{e^{16}}+$ &c. and extract the cube-root of e, and then multiply the said series into 4 times the said cube-root.

Now the cube-root of e is in this case $=\sqrt[3]{\frac{1}{6}}=\frac{1}{\sqrt[3]{6}}=\frac{1}{1.817,121}$; and consequently $4\sqrt[3]{e}$ is $=\frac{4}{1.817,121}$.

And,

And, since zz is $= \frac{1}{36 \times 3}$, and ee is $= \frac{1}{36}$, it follows that $\frac{zz}{ee}$ will be $= \frac{1}{3}$. Therefore $\frac{z^4}{e^4}$ will be $= \frac{1}{9}$, and $\frac{z^6}{e^6} = \frac{1}{27}$, and $\frac{z^{10}}{e^{10}}$ $(= \frac{z^6}{e^6} \times \frac{z^4}{e^4} = \frac{1}{27} \times \frac{1}{9}) = \frac{1}{243}$, and $\frac{z^{14}}{e^{14}}$ $(= \frac{z^{10}}{e^{10}} \times \frac{z^4}{e^4} = \frac{1}{243} \times \frac{1}{9}) = \frac{1}{2187}$. Therefore the series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} +$ &c. will in this case be $= \frac{C}{3} + \frac{G}{27} + \frac{L}{243} + \frac{P}{2187} +$ &c. $= \frac{.111,111,111, \&c.}{3} + \frac{.023,472,031, \&c.}{27} + \frac{.011,690,017, \&c.}{243} + \frac{.007,414,542, \&c.}{2187} +$ &c. $=$ .037,037,037,&c.+.000,869,334,&c.+000,048,107, &c. + 000,003,390, &c. + &c. = .037,957,868, &c.

Therefore $4\sqrt{^3}e \times$ the series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} +$ &c. is equal to $\frac{4}{1.817,121} \times$ .037,957,868, &c. $= \frac{.151,831,472, \&c.}{1.817,121} =$ .083,556,06.

Consequently the whole expression $\sqrt{^3}\overline{e+z} + \sqrt{^3}\overline{e-z} + 4\sqrt{^3}e \times$ the series $\frac{Czz}{ee} + \frac{Gz^6}{e^6} + \frac{Lz^{10}}{e^{10}} + \frac{Pz^{14}}{e^{14}} +$ &c. is $=$ 1.053,601,31 + .083,556,06 = 1.137,157,37; that is, the root of the proposed equation $x^3 - x = \frac{1}{3}$ is $=$ 1.137,157,37. Q. E. I.

ART. 12. This value of the root of the equation $x^3 - x = \frac{1}{3}$ is exact to six places of figures, the more accurate value of it being 1.137,158,164. We may therefore conclude that the expression here made use of to determine the value of x, to wit, $\sqrt{^3}\overline{e+z} + \sqrt{^3}\overline{e-z} +$

$4\sqrt{^3}e \times$

$4\sqrt{}^3e \times$ the infinite series $\frac{Czz}{ee}+\frac{Gz^6}{e^6}+\frac{Lz^{10}}{e^{10}}+\frac{Pz^{14}}{e^{14}}+$ &c. is somewhat preferable, with respect to the practical resolution of these equations, to the other expression of its value given in the former paper in the Philosophical Transactions, to wit, $\sqrt{}^3e \times$ the infinite series $2+\frac{2zz}{9ee}-\frac{20z^4}{243e^4}+\frac{308z^6}{6561e^6}-$ &c. For it appeared in Art. 42 of that paper, page 941, that the value of the root of this same equation $x^3-x=\frac{1}{3}$ obtained by computing four terms of the series $2+\frac{2zz}{9ee}-\frac{20z^4}{243e^4}+\frac{308z^6}{6561e^6}-$ &c. was 1.137,33; which is true only to four places of figures; whereas by computing the same number of terms of the series $\frac{Czz}{ee}+\frac{Gz^6}{e^6}+\frac{Lz^{10}}{e^{10}}+\frac{Pz^{14}}{e^{14}}+$ &c. we have just now obtained a value of the same root, to wit, the number 1.137,157,37, which is exact to six places of figures. This is agreeable to what was observed above in Art. 6.

A Summary of the Conclusions obtained in this Paper and the former Paper to which it is an Appendix.

ART. 13. I will now conclude this paper by setting down, in as concise a manner as I can, the several conclusions that have been obtained in this and the above-mentioned paper in the Philosophical Transactions for the

the year 1778, Number XLII. page 902, &c. concerning the root of the cubick equation $x^3-qx=r$, that the whole may be ſeen together at one view.

ART. 14. If $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, and e be put $=\frac{r}{2}$, and $ss=\frac{rr}{4}-\frac{q^3}{27}$, it is ſhewn in Art. 5, of the ſaid former paper, that the root of the equation $x^3-qx=r$ will be $=\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}\ \frac{+q}{3\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$, or $\sqrt[3]{e+s}+\frac{q}{3\sqrt[3]{e+s}}$.

ART. 15. And it is ſhewn in Art. 9. of the ſaid paper, that the root of the ſaid equation will in that caſe be alſo equal to $\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}\ \frac{q}{3\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}}$, or $\sqrt[3]{e-s}+\frac{q}{3\sqrt[3]{e-s}}$.

ART. 16. And it is ſhewn in Art. 11, of the ſaid paper, page 915, that the root of the ſaid equation will in that caſe be alſo equal to $\sqrt[3]{\frac{r}{2}+\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}+\sqrt[3]{\frac{r}{2}-\sqrt{\frac{rr}{4}-\frac{q^3}{27}}}$, or $\sqrt[3]{e+s}+\sqrt[3]{e-s}$.

ART. 17. And it is ſhewn in Art. 23, of the ſaid paper, page 923, that the root of the ſaid equation will in that caſe be alſo equal to $\sqrt[3]{e}\times$ the infinite ſeries $2-\frac{2ss}{9ee}-\frac{20s^4}{243e^4}-\frac{308s^6}{6561e^6}-\frac{1870s^8}{59049e^8}-\frac{111{,}826s^{10}}{4{,}782{,}969e^{10}}-\frac{2{,}358{,}512s^{12}}{129{,}140{,}163e^{12}}-\frac{120{,}646{,}960s^{14}}{8{,}135{,}830{,}269e^{14}}$

$\frac{120,646,960s^{14}}{8,135,830,269e^{14}}$ − &c. or (if we put the capital letters A, B, C, D, E, F, G, H, &c. for the several numeral coefficients 1, $\frac{1}{3}$, $\frac{1}{9}$, $\frac{5}{81}$, $\frac{10}{243}$, $\frac{22}{729}$, $\frac{154}{6561}$, $\frac{2618}{137,781}$, &c. of the terms of the series $1 + \frac{s}{3e} - \frac{ss}{9ee} + \frac{5s^3}{81e^3} - \frac{10s^4}{243e^4} + \frac{22s^5}{729e^5} - \frac{154s^6}{6561e^6} + \frac{2618s^7}{137,781e^7} -$ &c. which is equal to the cube-root of the binomial quantity $1 + \frac{s}{e}$), equal to $\sqrt{}^3 e \times$ the infinite series $2A - \frac{2Css}{ee} - \frac{2Es^4}{e^4} - \frac{2Gs^6}{e^6} - \frac{2Is^8}{e^8} - \frac{2Ls^{10}}{e^{10}} - \frac{2N^{12}}{e^{12}} - \frac{2P^{14}}{e^{14}} -$ &c; in which series all the terms after the first term 2A, or 2, are marked with the sign −, or are to be subtracted from the said first term.

ART. 18. And, if $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, but greater than its half, or $\frac{q^3}{54}$, and e be put, as before, $= \frac{r}{2}$, and $zz = \frac{q^3}{27} - \frac{rr}{4}$, it is shewn in Art. 31, 32, 33, 34, 35, of the said paper, pages 927—936, that the root of the equation $x^3 - qx = r$ will be equal to $\sqrt{}^3 e \times$ the infinite series $2 + \frac{2zz}{9ee} - \frac{20z^4}{243e^4} + \frac{308z^6}{6561e^6} - \frac{1870z^8}{59049e^8} + \frac{111,826z^{10}}{4,782,969e^{10}} - \frac{2,358,512z^{12}}{129,140,163e^{12}} + \frac{120,646,960z^{14}}{8,135,830,269e^{14}} -$ &c. or $\sqrt{}^3 e \times$ the infinite series $2A + \frac{2Czz}{ee} - \frac{2Ez^4}{e^4} + \frac{2Gz^6}{e^6} - \frac{2Iz^8}{e^8} + \frac{2Lz^{10}}{e^{10}} - \frac{2Nz^{12}}{e^{12}} + \frac{2Pz^{14}}{e^{14}} - \frac{2Rz^{16}}{e^{16}} + \frac{2Tz^{18}}{e^{18}} -$ &c; in which series the capital letters A, C, E, G, I, L, N, P, R, T, &c. denote the same numeral coefficients 1, $\frac{1}{9}$, $\frac{10}{243}$, $\frac{154}{6561}$, &c. as in the last Article, and all the terms that involve the

 odd

odd powers of the fraction $\frac{zz}{ee}$, to wit, $\frac{zz}{ee}$ itself, $\overline{\frac{zz}{ee}}\big|^{3}$, or $\frac{z^6}{e^6}$, $\overline{\frac{zz}{ee}}\big|^{5}$, or $\frac{z^{10}}{e^{10}}$, $\overline{\frac{zz}{ee}}\big|^{7}$, or $\frac{z^{14}}{e^{14}}$, $\overline{\frac{zz}{ee}}\big|^{9}$, or $\frac{z^{18}}{e^{18}}$, &c. are marked with the sign +, or are to be added to the first term 2A, or 2, and all the other terms are marked with the sign −, or are to be subtracted from the former.

These are the conclusions obtained in the said former paper, which is printed in the Philosophical Transactions for the year 1778, Number XLII. pages 902—949. The conclusions obtained in this paper are as follows.

ART. 19. If $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, but greater than its half, or $\frac{q^3}{54}$, and e be put, as before, $=\frac{r}{2}$, and $zz=\frac{q^3}{27}-\frac{rr}{4}$, it is shewn in the present paper, Art. 5 and 7, that the root of the equation $x^3-qx=r$ will be equal to the mixed expression $\sqrt[3]{e+z}+\sqrt[3]{e-z}+4\sqrt[3]{e}\times$ the infinite series $\frac{zz}{9ee}+\frac{154z^6}{6561e^6}+\frac{55913z^{10}}{4,782,969e^{10}}+\frac{60,323,480z^{14}}{8,135,830,269e^{14}}+$ &c. or $\sqrt[3]{e+z}+\sqrt[3]{e-z}+4\sqrt[3]{e}\times$ the infinite series .111,111,111, &c. $\times\frac{zz}{ee}+$.023,472,031, &c. $\times\frac{z^6}{e^6}+$.011,690,017, &c. $\times\frac{z^{10}}{e^{10}}+$.007,414,542, &c. $\times\frac{z^{14}}{e^{14}}+$ &c. or $\sqrt[3]{e+z}+\sqrt[3]{e-z}+4\sqrt[3]{e}\times$ the infinite series $\frac{Czz}{ee}+\frac{Gz^6}{e^6}+\frac{Lz^{10}}{e^{10}}+\frac{Pz^{14}}{e^{14}}+$ &c; in which series the capital letters C, G, L, P, &c. denote the

 same

ſame numeral coefficients $\frac{1}{9}$, $\frac{154}{6561}$, $\frac{55913}{4,782,969}$, $\frac{60,323,480}{8,135,830,269}$, &c. as they denoted in the two laſt Articles, to wit, the coefficients of $\frac{zz}{ee}$, $\frac{z^6}{e^6}$, $\frac{z^{10}}{e^{10}}$, $\frac{z^{14}}{e^{14}}$, $\frac{z^{18}}{e^{18}}$, &c. or of the odd powers of $\frac{zz}{ee}$ in the ſeries which is equal to $\sqrt[3]{1+\frac{z}{e}}$. And it is alſo ſhewn in Art. 6 and 12 of this paper, that this expreſſion, computed to a given number of terms, gives the value of x ſomewhat more exactly than the former expreſſion, $\sqrt[3]{e}\ \times$ the infinite ſeries $2+\frac{2zz}{9ee}-\frac{20z^4}{243e^4}+\frac{308z^6}{6561e^6}-$ &c. if computed only to the ſame number of terms.

ART. 20. As to the ſecond branch of the ſecond caſe of the equation $x^3-qx=r$, or that in which $\frac{rr}{4}$ is leſs than half $\frac{q^3}{27}$, or than $\frac{q^3}{54}$, I do not know any method of extending CARDAN's rule to it. But I have been informed by my learned and ingenious friend Dr. CHARLES HUTTON, Profeſſor of Mathematicks in the Royal Academy at Woolwich, that he has diſcovered ſuch a method: and I hope he will ſoon communicate it to this learned Society.

VI. *An Account of a Method for the ſafe Removal of Ships that have been driven on Shore, and damaged in their Bottoms, to places (however diſtant) for repairing them. By Mr.* William Barnard, *Shipbuilder,* Grove Street, Deptford; *communicated by* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read Dec. 23, 1779.

Deptford, April 14, 1779.

ON the ſhores of this Iſland, diſtinguiſhed for its formidable fleets and extenſive commerce, and ſo particularly ſituated, there muſt neceſſarily be many ſhipwrecks: every hint by which the diſtreſs of our fellow creatures may be alleviated, or any ſaving of property made to individuals in ſuch ſituations, ſhould be communicated for their good. As the members of the Royal Society have it in their power to make ſuch hints moſt univerſally known, I have been induced, from their readineſs to receive every uſeful information, to lay before them a particular account of the ſucceſs attending a method for the ſafe removal of ſhips that have been driven on ſhore,

ſhore, and damaged in their bottoms, to places (however diſtant) for repairing them; I hope, therefore, they will excuſe the liberty I have taken in preſenting this to them. Should the Society honour me by recording it, it will make me the moſt ample ſatisfaction for my attention to it, and afford me the greateſt pleaſure.

On January the 1ſt, 1779, in a moſt dreadful ſtorm, the York Eaſt Indiaman, of eight hundred tons, homeward bound, with a pepper cargo, parted her cables in Margate Roads, and was driven on ſhore, within one hundred feet of the head, and thirty feet of the ſide, of Margate Pier, then drawing twenty-two feet ſix inches water, the flow of a good ſpring tide being only fourteen feet at that place.

On the 3d of the ſame month I went down, as a ſhip-builder, to aſſiſt as much as lay in my power my worthy friend Sir RICHARD HOTHAM, to whom the ſhip belonged. I found her perfectly upright, and her ſhere (or ſide appearance) the ſame as when firſt built, but ſunk to the twelve feet water mark fore and aft in a bed of chalk mixed with a ſtiff blue clay, exactly the ſhape of her body below that draft of water; and from the rudder being torn from her as ſhe ſtruck coming on ſhore, and the violent agitation of the ſea after her being there, her ſtern was ſo greatly injured as to admit free acceſs thereto, which

which filled her for four days equal to the flow of the tide. Having fully informed myſelf of her ſituation and the flow of ſpring tides, and being clearly of opinion ſhe might be again got off, I recommended, as the firſt neceſſary ſtep, the immediate diſcharge of the cargo; and, in the progreſs of that buſineſs, I found the tide always flowed to the ſame height on the ſhip; and when the cargo was half diſcharged, and I knew the remaining part ſhould not make her draw more than eighteen feet water, and while I was obſerving the water at twenty-two feet ſix inches by the ſhip's marks, ſhe inſtantly lifted to ſeventeen feet eight inches, the water and air being before excluded by her preſſure on the clay, and the atmoſphere acting upon her upper part equal to ſix hundred tons, which is the weight of water diſplaced at the difference of thoſe two draughts of water.

The moment the ſhip lifted, I diſcovered ſhe had received more damage than was at firſt apprehended, her leaks being ſuch as filled her from four to eighteen feet water in one hour and a half. As nothing effectual was to be expected from pumping, ſeveral ſcuttles or holes in the ſhip's ſide were made, and valves fixed thereto, to draw off the water to the loweſt ebb of the tide, to facilitate the diſcharge of the remaining part of the cargo; and, after many attempts, I ſucceeded in an external applica-

tion

tion of fheep fkins fewed on a fail, and thruft under the bottom, to ftop the body of water from rufhing fo furioufly into the fhip. This bufinefs effected, moderate pumping enabled us to keep the fhip to about fix feet water at low water, and by a vigorous effort we could bring the fhip fo light as (when the cargo fhould be all difcharged) to be eafily removed into deeper water. But as the external application might be difturbed by fo doing, or totally removed by the agitation of the fhip, it was abfolutely neceffary to provide fome permanent fecurity for the lives of thofe who were to navigate her to the river Thames. I then recommended, as the cheapeft, quickeft, and moft effectual plan, to lay a deck in the hold, as low as the water could be pumped to, framed fo folidly and fecurely, and caulked fo tight as to fwim the fhip independant of her own leaky bottom. I herewith fend you a drawing of the fame, which will give the Society a clearer idea of the bufinefs than a long defcription, which, however, it may be ufeful to add for the better enabling others to put this method in practice.

Beams of fir timber, twelve inches fquare, were placed in the hold under every lower deck beam in the fhip, as low as the water would permit; thefe were in two pieces, for the convenience of getting them down, and alfo for the better fixing them of an exact length, and well

bolted

bolted together when in their places. Over theſe were laid long Dantzic deals of two inches and an half thick, well nailed and caulked. Againſt the ſhip's ſide, all fore and aft, was well nailed a piece of fir, twelve inches broad and ſix inches thick on the lower, and three inches on the upper edge, to prevent the deck from riſing at the ſide. Over the deck, at every beam, was laid a croſs piece of fir timber, ſix inches deep and twelve inches broad, reaching from the pillar of the hold to the ſhip's ſide, on which the ſhores were to be placed to reſiſt the preſſure of the water beneath. On each of theſe, and againſt the lower deck beam, at equal diſtance from the ſide and middle of the ſhip, was placed an upright ſhore, ſix inches by twelve inches, the lower end let two inches into the croſs piece. From the foot of this ſhore to the ſhip's ſide, under the end of every lower deck beam, was placed a diagonal ſhore, ſix inches by twelve, to eaſe the ſhip's deck of part of the ſtrain by throwing it on the ſide. An upright ſhore, of three inches by twelve, was placed from the end of every croſs piece to the lower deck beams at the ſide; and one of three inches by twelve on the midſhip end of every croſs piece to the lower deck beam, and nailed to the pillars in the hold. Two firm tight bulkheads or partitions were made as near the extremes of the ſhip as poſſible. The cieling

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or infide plank of the fhip was very fecurely caulked up to the lower deck, and the whole formed a compleat fhip with a flat bottom within fide to fwim the outfide leaky one; and that bottom being depreffed fix feet below the external water, refifted the fhip's weight above it, equal to five hundred and eighty-one tons, and fafely conveyed her to the dry dock at Deptford.

Since I wrote the above account, I have been defired to ufe the fame method on a Swedifh fhip, ftranded near Margate on the fame day as the York Eaft India-man, and fwim her to London. As this fhip is about two hundred and fifty tons, and the execution of the bufinefs fomething different from what was practifed with regard to the large fhip, I hope it will not be thought improper to defcribe it.

As this fhip's bottom was fo much injured, having loft eight feet of her ftern-poft and all her keel, feveral floor-timbers being broke, and fome of the planks off her bottom, (fo as to leave a hole big enough for a man to come through) feveral lower deck beams being likewife broke, and all the pillars in the hold broken and wafhed away; I thought it neceffary to connect, in fome degree, the fhattered bottom with the fhip's decks, not only to fupport the temporary deck by which fhe was to fwim up, but to

 prevent

prevent the bottom being cruſhed by the weight of the ſhip when ſhe was put upon blocks in the dry dock: to effect which, after I had put acroſs twelve beams of fir, ſix inches by twelve, edgeways, one under every lower deck beam of the ſhip, and well faſtened them to the ſhip's ſide, I placed two upright pieces to each beam of ſix inches by twelve, ſecurely bolted to the ſides of the keelſon, and ſcored ſix inches under the ſhip's lower deck beams, and three inches about the beams of the temporary deck, and well faſtened to each: then the deck was laid with long two-inch Dantzic deals, and well nailed and caulked; the ſhip's inſide plank was well caulked up to the lower deck. A piece of fir, of twelve inches broad and two inches thick on the upper, and four inches on the lower edge, was well nailed to the ſhip's ſide all fore and aft, and well caulked on both edges to prevent the ſide of the deck from leaking, or being forced up by the preſſure of the water againſt the deck, a two-inch deal or croſs piece was laid over every beam from the ſhip's ſide to the uprights at the middle line; then, at equal diſtance from the ſide and middle line, pieces of ſix inches ſquare, as long as could be got down, were put all fore and aft on both ſides, ſcored two inches over every croſs piece, and well bolted through the croſs piece and deck, and into the fir beams. From this fore and aft piece or ribband to the ſhip's

ſhip's ſide, and from it to the uprights in the middle, were placed two rows of diagonal ſhores, ſix inches ſquare, the heels of which were ſecurely wedged againſt the fore and aft piece or ribband, which afforded ſufficient ſupport to the temporary deck without any other ſhores. Two bulkheads or partitions were built, as far as the fore-maſt forward, and mizen maſt aft, well planked, ſhored, and caulked, to reſiſt the water. As decks laid in this manner, and in ſo much hurry as the time of low water requires, will of conſequence leak in ſome degree, and as that leakage, waſhing from ſide to ſide, will cauſe the ſhip to lay along, I fixed a two-inch deal, twelve inches broad, edgeways, all fore and aft at the middle line, and well caulked it, to ſtop half the water on the weather or upper ſide, when the ſhip would incline either way, which not only made her ſtiffer under ſail, but facilitated the pumping out the water made by leaks in the deck.

This deck was ſixty-three feet long and twenty-three feet broad, and was laid at five feet five inches above the bottom of the keel, or four feet above the top of the floor timbers, and ſwam the ſhip at twelve feet five inches water, reſiſting two hundred and ſixteen tons, and containing under it one hundred and twenty-four tons of water, which preſſing againſt the under ſide of the temporary

 deck

deck acted as ballaft, and brought her fafely into the dry dock at Deptford, from the moft dangerous fituation poffible, being partly within and partly without Margate Pier, where fhe had been left by fome Ramfgate men, who had undertaken to remove her from the place where fhe was ftranded to a fafer one within Margate Harbour.

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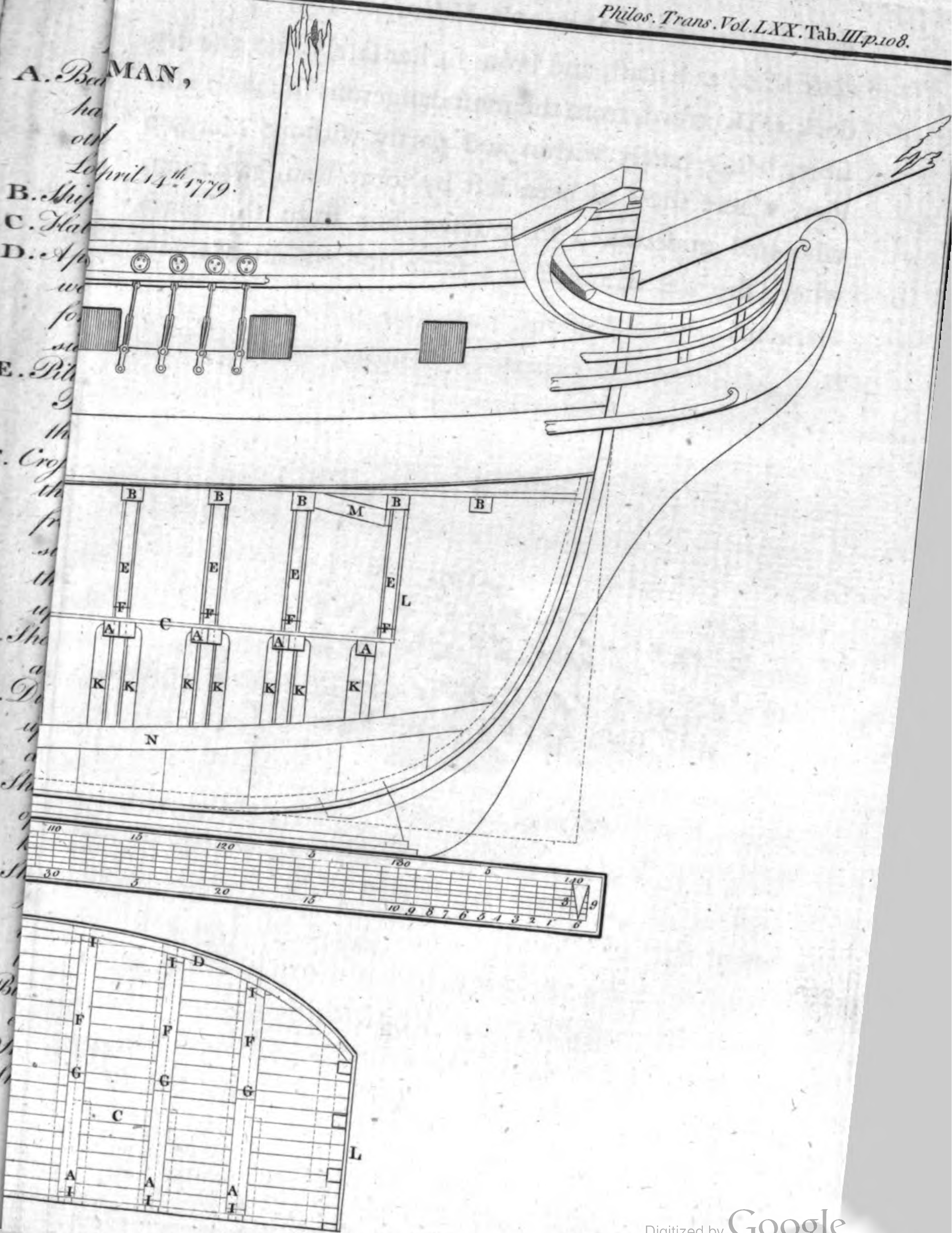

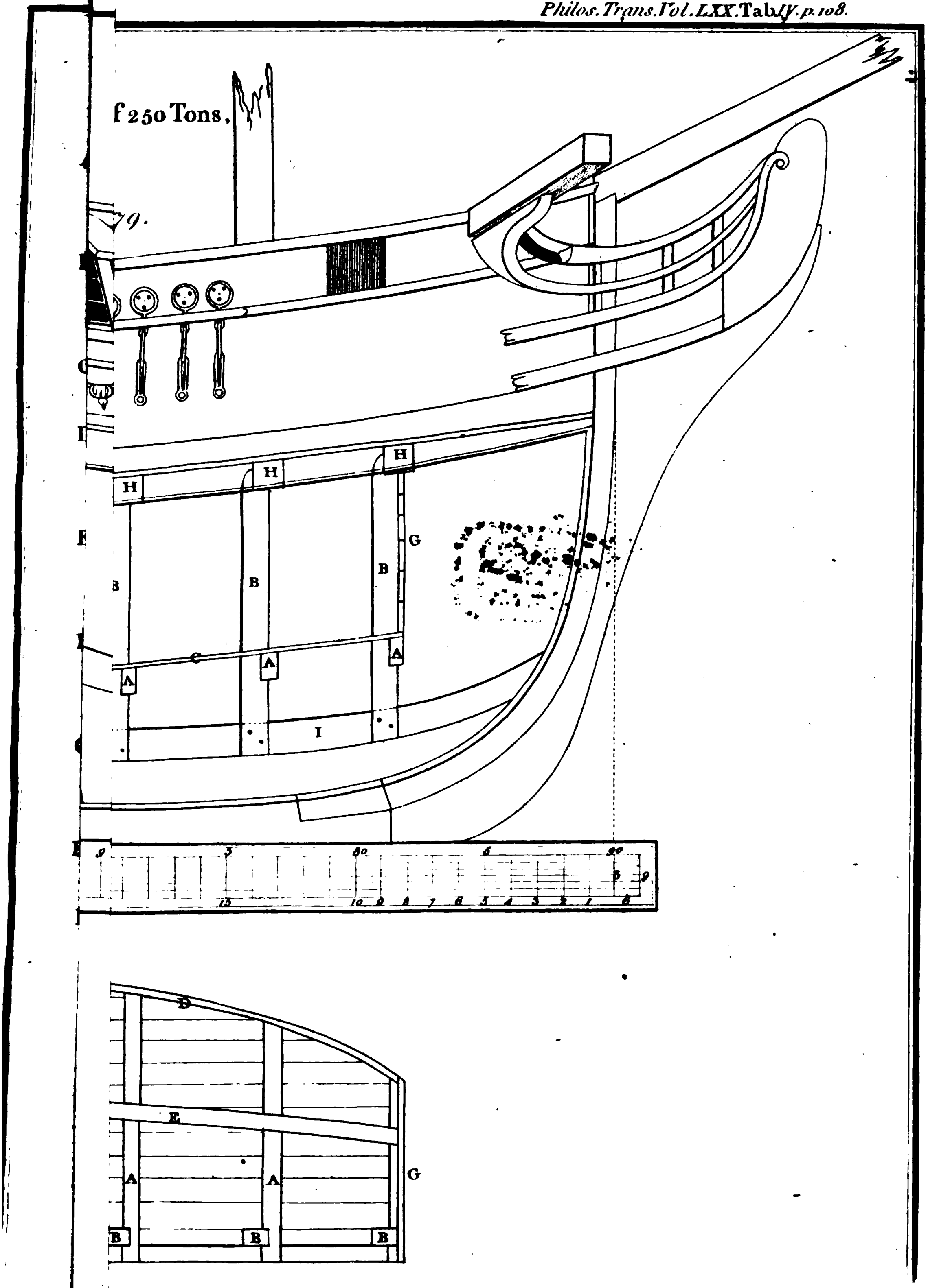

Bas.

VII. *Experimenta quædam novum acidum Animale ſpectantia.* *Autore* F. L. F. Crellio, *M. D. et Prof. Chemiæ,* Helmſtadienſi; *communicated by* William Hunter, *M. D. F. R. S.*

Read Jan. 13, 1780.

SEPTEM nunc fere elapſa ſunt luſtra, ex quo perill. J. A. DE SEGNER, vir inter mathematicos celeberrimus, novum acidum, idque animale, atque ex pinguedine erutum[(a)] detexit, eoque, qualia incrementa ab illo ſperare potuiſſet chemia, ſi glorioſe inchoatam viam proſequutus eſſet, pleniſſime patefecit. Acidi naturam penitius introſpiciendi deſiderium non movit alios, poſt SEGNERUM chemicos ad plura capeſſenda pericula, quum meſſis forſan non ſatis larga relicta illis videretur; ſed et ſparſos adhuc colligere flores prementi tanti viri veſtigia profuiſſet.

Præcipua SEGNERI experimenta hæc ſunt. Sebum bovillum colatum leni igne arenæ balneo diſtillavit, præpoſitis in excipulo aquæ ℥ iij; olei, quod ſub vaporibus oculos acerbiſſime pungentibus prodibat, cum ℥ ij tranſiiſſent,

(a) Diſſ. Inaug. de acido pinguedinis animalis; præſide O. A. SEGNERO, Reſpond. D. H. KNAPE. Gott. 1754.

excipulum

excipulum mutavit, nova præposita aqua. Iterum olei ℥ ij evocavit; tunc aliud vas, aqua repletum de novo apposuit; continuato hoc modo opere, donec aqua non amplius saporem acidum referret. Quæ in excipulo inveniebantur, ex oleo, partim coacto, partim fluido, et aqua constabant. Hæc calori per aliquot dies exposita, concussa, per infundibulum separabat: aqua obtenta odoris saporisque erat pungentis, et cum sale alcalino effervescebat; quod quidem acidum, in aqua degens, ab hujus parte superflua liberabat, distillando ad dimidium, vel quousque aqua saporis atque odoris expers esset; residua portio quo diutius operatio continuatur, eo majori acredine prædita transtillat. Quo majorem puritatis gradum acquirat acidum, cum sufficienti quantitate salis alcalini saturandum, hoc lixivium evaporandum, tunc addendum est oleum vitrioli ea quantitate, ut dimidium sit ponderis assumti salis alcalini. Qui tunc transit super alembicum liquor, purus est, limpidus et valde acidus, tantisper tamen adhuc oleosus. Quod oritur ex combinatione hujus acidi et salis alcalini vegetabilis sal medium, ad terram foliatam tartari accedit, et spiritum vini eodem modo tingit; integre tamen in eo non solvitur: fixum est, nec super carbonibus crepitans, nec ignem concipiens. Ex analogia technologiæ chemicæ nomen tartari animalis illi imponere fas mihi videtur.

Alcali

Alcali volatile, huic acido nuptum, ſal conſtituit ſapore ſalis ammoniaci, relinquens in lingua ſenſum refrigerii; igne ſublimatur in flores niveos: nomen ſalis ammoniaci animalis meretur.

Hæc ſunt præcipua, quæ de hujus acidi, e pinguedine bubula (ut cætera taceam) eruti, analyſi nobis tradidit SEGNERUS. Superſunt adhuc quædam nondum tentata experimenta, quæ naturam illius interius declarant, et quæ nunc tua pace, vir celeberrime, tibi referam. Ex antea enim dictis nondum patet proportio partium conſtituentium, olei ſcilicet, acidi, et terræ in pinguedine; quam ut eruerem ſequenti modo rem aggreſſus ſum.

EXP. I. Sumſi lardi bubuli, fuſi, colatura a rebus peregrinis liberati, libras II; retortam vitream ad dimidiam illius partem illo ſolo replevi, atque commiſſuris vaſorum bene clauſis expoſui igni reverberii in arenæ balneo. Cum ſebum omne fuſum eſſet, tranquille diſtillabat, non valde ſpumeſcens, nec ultra unam quartam partem ſpatii, quod antea occupabat, ſeſe extendens. Primo prodibat oleum tenue, quod fluidum remanebat; tunc acidum fundum petens, et cum illo oleum, quod igne debiliori, utut ex retorta cadebat, protinus coaluit; illo vero adaucto, ut gutta guttam truderet, hoc iterum fundebatur, in excipuli autem fundo paulo poſt iterum denſatum. Fluidum, ut omne tranſcenderet (quod 16 horis factum eſt) ignis

 requirebatur

requirebatur vehementior, sic ut fundus catini ferrei bene ignesceret. Vasis frigefactis magna pars olei transtillati coacta erat: excipulo aperto odor surgebat haud fere tolerabilis, et nares, et oculos vehementissime vellicans, sic ut animam fere intercluderet. Fluidum decantatum ℥ iij ß, scrupul. semis æquabat, quod ex duobus liquoribus diversi generis consistens, ope infundibuli separabatur. Oleum viridiusculum, olei absinthii colorem tenens, ℥ j, ʒ vij, scrupul. ij librabat: ponderosius acidum, aurei coloris, pungentissimum, ℥ j, ʒ iij ß. Residuum oleum coactum, lardo suillo simile, odore quidem pungente adhuc præditum erat, qui vero illi non proprius ab acido non penitus separabili ortum ducebat. In retorta remanebat carbo, concussu a parietibus secedens, per fragmenta e vase educendus, ad tactum lævis, coloris splendentis, ponderis ℥ j, ʒ iv ß.

EXP. II. Oleum coactum in excipulo, ut funderetur, ignem requirebat non lenem sed tamen non adeo fortem, quam qui sebum ipsum liquefecerat; quod prout diffluebat, in retortam infundebatur. Ignis ad hanc distillationem adhibitus erat inter 430—450° therm. FAHRENH. quo gradu, cum nihil amplius transiret, operatio finiebatur. In excipulo iterum inveni oleum coactum, et quod supernatabat fluidum, ponderis erat ℥ v, ʒ vj, odoris non adeo penetrantis, quam in priori experimento maxima

4. parte

parte ex oleo aurei coloris constans parum acidi admixtum erat: $\frac{1}{18}$ – $\frac{1}{20}$.

EXP. III. Oleum densatum minori, ut in exp. 2. liquescebat calore; quo facto addebatur residuæ ab illo in retorta massæ, ejusdemque ignis gradui committebatur; quo eadem oriebantur phænomena, quæ modo descripsi. Fluidi accepi ℥ vj, ʒ j ß, quod pallidioris erat coloris, et flavedine sulphuris æmula tinctum; parca iterum erat acidi portio.

EXP. IV. Portioni quæ isti caloris gradui resistebat, addebatur, liquefactione prægressa, oleum densatum, atque igni reverberii exponebatur. Vaporum pars quædam lutum, ex farina et aqua confectum, penetrabat, qui candela adhibita flammam concipiebant cœrulescentem, at adeo debilem, ut chartam luto illinitam, vasorum commissuras occludentem lædere non valeret. Quibus vero rursus prorumpentibus vaporibus novum super impositum lutum exitum mox cohibebat. Distillatio ad siccitatem usque protracta dabat multum olei coacti, et paululum fluidi, cuique pondus erat ℥ j, ʒ v ß; acidi huic admixti exigua erat portio, $\frac{1}{8}$ fere, cuique color valde pallidus, oleum vero intensius erat tinctum, atque ex rubro flavum. Olei pars coacti cum mollis esset, quidpiam olei fluidi, et acidi evoluti retinuit, fluidissima modo parte decantata. Carbo fundum occupans, ponderabat ʒ vj, ℈ ij, eique in exp. II. obtento perfecte ceterum similis.

EXP. V. Liquefacto exper. antecedentis oleo, ignis ſubjiciebatur diſtillatorius, ejuſdem gradus, ut in exp. 2. et 3. Obtinui ℥ ij fluidi, cuique pars circiter $\frac{1}{14}$ acida erat, parum colorata, fere alba; oleum vero coloris ſaturate ſulphurei.

EXP. VI. Quod denſatum erat oleum, leni calore recuperavit fluiditatem (quæ quidem poſt quamvis peractam diſtillationem ſemper imminutum ignem poſcebat, utut ſcilicet oleum mollem magis naturam acquirebat): reſiduæ in vaſe retorto maſſæ additum, eandem caloris vehementiam ſuſtinuit, ac in exp. 5. atque dedit fluidi ℥ j ſs ejuſdem coloris ac naturæ, ut antecedens.

EXP. VII. Reſiduum nunc igni reverberii expoſui, addito antea oleo coacto exp. 6. Quod obtinui fluidum, ponderis ℥ ij, ʒ v ſs erat, cujus pars acidi parca flava, oleoſa vero ex rubro fuſca erat. Carbo ℥ j, ʒ vij.

EXP. VIII. IX. Seboſa materia fuſa, ex retorta diſtillabatur iterum reverberii igne; quo facto, in excipulo inveniebam ℥ j, ʒ ij, olei ſaturatioris quam antecedens, cui parciſſima acidi flavi quantitas admixta erat. Quum reſidua in vaſe maſſa, nondum in carbonem converſa eſſet, ſed oleo adhuc ſcatens; addidi ei parvam illam olei coacti quantitatem, iterumque ad ſiccitatem uſque diſtillavi, atque obtinui olei fuſci ℥ ſs una cum acidi tantillo, cui innatabat parca quantitas olei coacti, quod ad pondus ℈ j

accedere

accedere mihi videbatur: modico calore applicato, oleo fluido ſeſe admiſcebat; quo vero calore orbatum iterum ex illo ſecedebat ſub forma, terræ foliatæ tartari ſimili: increſcente vero paulo poſt atmoſpheræ calore, hoc cum illo ſemper intimo connubio junctum deprehendebatur. Reſiduum ex hac diſtillatione erat carbo, pond. ʒ vj ß.

Hac itaque methodo tota lardi bubuli quantitas in ſuas partes reſoluta erat; reſtabat adhuc, ut, quæ oleo admixta erat portio acida, ab eo ſepararetur.

EXP. X. Confudi itaque quæcunque obtinui diſtillationis ope e ſebo fluida, atque ope infundibuli ſeparavi acidi, aureo ſplendore nitentis, ℥ iij, ʒ v, olei vero, cui color erat ex rubro fuſcus, ℥ xxj ß. Si iſti ponderi fluidorum, ſcil. ℥ xxv, ʒ j, additur iſtud carbonum in diverſis operationibus obtentorum, nempe ℥ v, ℈ ij, apparet, de tota ſebi maſſa, omni quamvis cura adhibita, deperditas eſſe ℥ j, ʒ vj, ℈ j; quod vero mirum non erit perpendenti, tot fuiſſe diſtillationes, tot olei coacti liquefactiones, quæ avolandi occaſionem haud facile impediendam fluidis dederunt; partem præterea quoque harum, vaſorum parietibus tenacius adhærentem ex illis effundi non potuiſſe.

Oleum etſi accuratiſſime quantum ope mechanica licet, ab acido ſeparatum, cum annotante SEGNERO, partem tamen hujuſce abſcondat, hoc ex illo elicere omni ſtudio allaboravi.

EXP. XI. Adjeci itaque omni oleo aquæ portionem æqualem; tunc illud calori digestionis exposui atque sæpius concussi; quo facto aqua saporem acidiorem acquirebat, et addito sale alcalino effervescebat. Atque hæc omnia toties repetii, donec aqua nullo amplius sapore gauderet, nec cum alcalinis confligaretur.

Sed accuratius paulo investiganda erat quantitas hujus in oleo latentis acidi; quem in finem sequenti modo rem confeci.

EXP. XII. Salis tartari puri ʒ ß adieci acidi exp. 10. quantum ad illius plenariam saturationem requirebatur, eoque modo consumsi hujus ʒ vj ß; quod vero sal medium pro norma instituendæ computationis mihi paravi.

EXP. XIII. Aquæ itaque acido impregnatæ exp. 11. injeci sal tartari, cujus ʒ ij ad omnis acidi absorptionem vix sufficiebant. Si itaque hujus quantitatem ad ductum exp. 13. computo, accedet ad ℥ iij, ʒ ij.

EXP. XIV. Olei ex rubro fusci, aqua edulcorati (exp. 11.) ℥ iij, adiecta iterum pari portione aquæ distillatæ, igni lampadis exposui, et obtinui, ante transitum hujus, oleum limpidum odoris et saporis, qualis fere est, olei ætherei: deinde cum aqua quoque transibat olei albi portio; postquam vero illa penitus transcenderat, lampadem removi, et ℥ iij olei separavi ab aqua, acidulo sapore impregnata.

EXP.

EXP. XV. Eidem oleo toti (exp. xi.) per ſe lampadem ſuppoſui, et par (exp. 14.) oleum limpidum elicui: in fundo hærebat acidi rubeſcentis parca portio.

EXP. XVI. Hoc oleum exp. 15. in vaſis circulatoriis paulo vehementiori igni expoſitum, ſaturatiorem pedetentim acquirebat colorem, donec in fuſcum badium tranſiret[b].

EXP. XVII. Reſiduum ex diſtillatione exp. 15. fortiori igni expoſui, donec omne tranſtillaſſet, quod erat coloris aliquantulum dilutioris, quam ante operationem. Ignis vehementiæ reſiſtebat pars, quæ retorta diffracta, carbo erat ponder. ℥ iij, ʒ xviij, perfecte ſimilis illi exp. 1. 4. 7. 9.[c].

EXP. XVIII. XIX. Olei (exp. 14. 15.) ʒ iij in ſpiritus vini ℥ ij inſtillavi, cujus dimidiam partem in vaſis circulatoriis leni igne digeſſi; alteram in retorta diſtillavi: prior partem olei ſupernatantem non ſolutam monſtrabat; poſterior ſoluta erat.

EXP. XX. Utrique addidi aquam, quo facto mixtum colore erat lacteſcente, quem cum amiſiſſet, claraque evaſiſſet, oleum ſuperficiem petiit.

EXP. XXI. XXII. Olei rectificati (exp. 14.) ebullientis ℥ j inſtillavi ſalis tartari cauſtici calidique ʒ j, quod fere to-

(b) Idem, analogia ductus tentavi cum oleo animali Dippelii, et vehementiori igne eandem mutationem paſſum eſt.

(c) Collatis inter ſeſe exp. 10. 13. 17. et computo ducto, apparebit, ineſſe ſebi lb. ij, olei puri ℥ xiv, acidi ℥ vij, ℈ ij, carbonis ℥ x, ʒ vj, ℈ j.

tum

tum citissime absorbebatur; adjeci adhuc ℥ j ß, quæ ad digiti unius altitudinem salem tegebat. Digestum horarum 2 spatio, in saponem conversum erat; sic dicto nigro similem [(d)]. Eadem res cum oleo exp. 11. æque felici fere negotio cessit.

EXP. XXIII. XXIV. Spiritum salis ammoniaci cum calce viva paratum adjeci oleo rectificato; eodem momento lactescebat, solutæque sic saponis speciem constituebat. Cum oleo exp. 11. idem prorsus simili rependebatur successu.

EXP. XXV. XXVI. Idem tentavi cum sale alcalino volatili crystallisato, quod injeci oleo rectificato; sal in fundo hærebat haud solutum. Applicato igne illud collum vasis petiit, nec combinationem iniit cum oleo. Nec feliciori successu idem cum sale in parcissima aquæ quantitate soluto sum periclitatus.

EXP. XXVII. XXVIII. Oleum vitrioli album oleo exp. 14. instillavi; pars coagulata picea a supernatante fluidiori mox resolvebatur, fluidumque sistebat atro fuscum, odoris olei fere canabini rancidi. Oleum totum eadem phænomena edidit, nisi quod pars coagulata non integre soluta erat. Utrique aquam adjeci, qua omnia dissolvebantur,

(d) Sic itaque facillimo negotio saponem sic dictum Starkeaynam confeci, qua methodo cum aliis oleis æthereis in 2 dierum circiter spatio, parem licet componere saponem, quamvis alio modo sit res difficillima.

saponis soluti forma. Alcali adjecto, oleum nigricans superficiem petebat; fluidumque evadebat pellucidum.

EXP. XXIX. XXX. Spiritum nitri fumantem, tertia parte olei vitrioli auctum, adieci oleo albo: nullus fumus, nec flamma, nec combinatio, aut condensatio. Spiritus nitri inferiora occupabat, colorem perdidit intensiorem, et stramineum acquisivit; superiora occupans oleum e contrario coloris erat aurantii saturati.

EXP. XXXI. XXXII. XXXIII. Oleum exp. 10. cum eodem nitri acido mixtum, fumum edebat, coagulabatur, et particulæ quædam descendebant, quæ carbonaceæ videbantur. At fluido decantato aqua adjecta solvebantur, et liquidum constituebant coloris straminei, saporis amarulenti. Evaporata aqua, concrementum accepi flavum, facie quadam salina lamellata, quod solutum, spiritum vini colore induebat aureo. Sale tartari, solutioni aquosæ adjecto, nulla olei evenit separatio.

EXP. XXXIV. XXXV. Spiritus salis fumans, mixtus cum utroque oleo, amice se illi junxit, nec mutationem fere ullam produxit; parca modo illius quantitate coacta, quæ a fluidis liberata, aëri exposita, purpureum acquisivit colorem. Oleum superficiem tegens, spiritui qualitatem fumantem diu conservavit, quamvis in vitro vini potui destinato, aëri accessui exposita erant.

EXP. XXXVI. XXXVII. Spiritus veneris concentratiffimus (viridi vero imbutus colore) mixtus oleo rectificato, nullo modo in illud agebat; fed calori expofita, ut ebullirent, priftinum colorem exuebant; albumque antea oleum nunc gaudebat fufco dilutiori; viride vero acidum fufco utique, at faturatiori; fedimentum nullum apparebat. Fluidum odorem penetrantiffimum, illi fimilem, quem in exp. 10. defcripfi, et tunc adhuc edebat, fi acidum ab oleo, mediante infundibulo feparatum erat. Odor itaque ab oleo, concentrato acido intime nupto, provenire videtur.

EXP. XXXVIII. Carbonis, qui olei diftillatione peracta remanfit, natura adhuc inquirenda eft, quod ut fieret, incinerationem tentavi. At enim vero hæc difficillime ceffit; per plures enim horas in crucibulo capaci aperto, qui totus candebat, carbonem detinui, et vix ullam mutationem paffus eft. Confici itaque curavi ex materia lateritia vas planum, parvo limitatum margine, pediculo intra prunas, furnum anemium occupantes, elevatum, ut flamma fuperficiem illius, carbone comburendo tectam, lambere poffet. Hoc modo, diu tamen continuata calcinatione, in cineres converfus eft, atque de ℥ ij remanferunt ʒ iij.

EXP. XXXIX. Cineri, qui rubrum quemdam colorem induit, adjeci aquæ diftillatæ ℥ ij: leni igne digeffi; et

 colavi,

colavi. Sapore instructa erat salino; qua vero evaporata ad cuticulam, nullæ apparebant crystalli; continuata vero ad siccitatem evaporatione obtinui grana 41 salis, incertæ prorsus figuræ, et singularis saporis; in aëre non deliquescebat.

EXP. XL. Quum experimentis clarissimi GAHN, quibus phosphorum novo parare modo docet, constet, inesse animalibus sal medium terreum, ex acido phosphori et terra calcaria compositum, an sal nostrum fixissimum ejusdem naturæ sit, inquirere animo occurrit. Soluto itaque in aqua distillata, addidi guttas aliquot acidi vitriolici, quo protinus moleculæ albæ descendebant, manifesto indicio, huic sali terram infuisse calcariam, quæ illi acido jungi maxime omnium amat.

EXP. XLI. Præcipitatum, a fluido, per filtrum separatum, liberavi aqua, et obtinui salem albidum acidi saporis, quod fistula ferruminatoria in vitrum pellucidum abiit. In aqua solutum, ad consistentiam mellis evaporatum cum fuligine tedæ miscui, retortæ terræ loricatæ injeci, atque eidem ignis gradui, quo phosphorum parare solent, commisi. Tribus vix horis præterlapsis lætus vidi collum retortæ, quod aquæ proximum erat lumine phosphorico conspicuum, quod per horam et ultra substitit; at phosphorum sub aqua (ob parcam massæ quantitatem) non obtinui.

EXP. XLII. Cineribus ſale orbatis (exp. 39.) ſiccatis, adjeci aquæ fortis ℥j ut ea terram calcariam, et præcipue ferrum, quod adeſſe ex colore ſuſpicatus ſum, extraherem. Per 24 horas leni igne digeſſi: cineres non eo quidem amplius gaudebant colore, at nitri acidum non illo indutum erat, quo ſolutio ferri eſſe ſolet; ejuſque guttæ, infuſioni gallarum inſtillatæ, nec colorem ad atrum vergentem producebant. Ruber iſte itaque color non a marte, ſed a principio inflammabili, ut ſæpe fieri ſolet, ortum duxit.

EXP. XLIII. Liquori illis fœto, quæ ex cineribus extraxerat aqua fortis inſtillavi vitriolicum acidum, quo facto turbidus evaſit, et ad fundum particulas depoſuit ſeleniticas; quibus elucet, terram adhuc adfuiſſe calcariam nulli acido unitam. Hiſce filtro ſeparatis, reliquum evaporavi; aſt omne in auras abiit.

EXP. XLIV. Quæ nitri acidum ſolvere haud potuit, ea edulcoravi, exſiccavi; ponderis erant ʒ ij. Hiſce affudi ℥j olei vitrioli, forti igne omne fluidum evocavi; tunc cum aqua diſtillata coxi. Fluidum, peracta filtratione, evaporavi; quo facto obtinui cryſtallos parvas, ſaporis aluminoſi, quæ terram e ſale ammoniaco præcipitabant, eamque ipſæ, adjecto ſale alcalino, dimittebant.

EXP. XLV. Edulcoratum, exſiccatumque reſiduum æquali portione ſalis alcalini mineralis junctum fudi in

crucibulo

crucibulo forti igne; atque obtinui optimæ notæ vitrum pellucidum. Supereſt adhuc ut acidum, ejus naturam, actionemque in alia corpora examini ſubjiciam.

EXP. XLVI. Acidum exp. X. leni calori expoſui, et diſtillavi; quod tranſibat, coloris erat albidi ad flavedinem vergentis; in fundo remanebat maſſa carbonacea, quodammodo unguinoſa. Tentamen, quod inſtitui, ut phlegmate, leni igne evocato, fortius obtinerem acidum, felici deſtitutum erat ſucceſſu; liquidum enim in excipulo ejuſdem erat acoris, ac illud in retorta.

EXP. XLVII. De ſale, ex alcali vegetabili et acido noſtro orto, jam ſupra, et exp. 12. retuli: ſupereſt adhuc aliud fixi generis. Affudi nempe ſalis alcalini mineralis cryſtalliſati ʒ iij acidum exp. 46. ad plenariam ſaturationem; eoque modo conſumſi ℥ v. Evaporavi ad cuticulam uſque; ſed cum cryſtalli fuſco gauderent colore omnem humiditatem expuli; fudi leni igne quo uſque fumum non amplius ederet, aut particula, e crucibulo hauſta, ſolutionem in aqua redderet limpidam, et carbonaceam materiam ad fundum deponeret. Tunc maſſam ſalinam ſolutam ad cryſtalliſationem diſpoſui, atque obtinui maſſam ſalinam, ex cryſtallis quadratis, in pyramidem triangularem ut plurimum deſinentibus, compoſitam, quæ aëri expoſita pulvere albo farinoſo tegebatur.

EXP. XLVIII. Saturando ſali alcalino volatili ℥ x acidi impendi. Leni igne in cucurbitula capitello inſtructa expoſitum, aquoſos, odoris et ſaporis expertes vapores edidit, nec in altum tolli ſe ſivit, niſi eodem fere ignis gradu adhibito, quo ſal ammoniacum commune elevari ſolet. Flores tunc capitello et cucurbitæ collo ſe apponebant albiſſimi.

EXP. XLIX. Terra calcaria ſolvebatur non ſine inſigni effervefcentia, ut ejus ℥ xj ab acidi noſtri ℥ ij ſuſciperentur. Idem ſal paravi, miſcendo ſebum cum calce viva pulveriſata, illud leni igne fundendo; tunc cum aqua coxi; et colatura evaporata concrevit in maſſam ſalinam. Melius adhuc res ceſſit, cum ſebum calce mixtum, diſtillarem. Lixivium ad debitam conſiſtentiam evaporatum cryſtallos ſtitit fuſcas, quas itaque fudi (ut in exp. 46.) tunc aqua diſtillata ſolvi, ſuperfluam expuli, et obtinui cryſtallos pellucidas hexagonas, plana terminatas ſuperficie. Sapor acris ſalſus eſt, ſed non adeo urens, ut ſal ammoniacum fixum: in aqua facile ſolubilis, aëre vero non deliqueſcens; ſpiritui vini non obtemperat. Salis calcarii animalis nomen non ineptum videtur.

EXP. L. Effervefcendo magneſia alba quoque ſolvitur, et ℥ ix, acidi ℥ ij ſaturaverant. Mixtum omni quamvis data opera nullam formam cryſtallinam induit; potius

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maximam partem in maſſam gummoſam concrevit, quæ aëri expoſita deliqueſcebat ſaponiſque erat amarulenti.

EXP. LI. Terra aluminis non abſque difficultate acido noſtro ſe junxit: hujus enim ʒ ij addebam præcipitati, ex alumine et ſale alcalino fixo nati, edulcorata adhuc madidi (e) ʒ ß; ſolutum quidem in initio omne crederes; at paulo poſt magna pars (dimidia ſcilicet ut videbatur) fundum petiit. Colatum fluidum in cryſtallos coire denegabat; ſapor erat adſtringens, non dulceſcens, potius auſterus. Terram ſolutam demonſtrabat adjectum alcali, quo addito præcipitabatur continuo. Nomen aluminis animalis imponendum ei videtur. Affinitatem non arctam intercedere terræ aluminoſæ cum noſtro acido, probat commixtio ſalis calcarii animalis cum ſolutione aluminis ſaturata; quo facto, ne ipſa coctione adhibita, ſelenites nulla exoriebatur; quæ vero copioſiſſima eſt, ſale ammoniaco fixo alumini addito.

EXP. LII. Terræ, e liquore ſilicum addito vitrioli acido ortæ, ſcrup. dimidio adjeci ʒ ij acidi; ſed nulla ejus videbatur in illam actio, nec digeſtione et coctione conſpicua. E liquore colato ſal alcalinum fixum nihil exturbabat; ut itaque vim in terram vitreſcibilem nullam illi ineſſe, veri ſimillimum ſit.

(e) Siccatam terram nempe aggredi non videtur; et ʒ ß, omni humiditate expulſa ponderis erat granor. iv.

EXP.

EXP. LIII. Quæ vero omnia corporum acido nubendorum tentamina et eum præcipue in finem inſtituta erant, ut acidum noſtrum concentretur, et eo in ſtatu ex illis expellatur. Placet inſtar omnium nunc ſalis ex acido et alcali vegetabili conflati examen proponere. Ejus ʒ xij adjeci olei vitrioli ℥ ß, et leni igne acidum noſtrum expuli, quod vaporum griſeorum forma tranſiit, eamque ſemper conſervavit. Aquæ inſtar limpidum erat, valde acidum, ponderis ℥ ß. Si ſal medium ſatis calcinatum haud eſt (exp. 46.) acidum prodiens quidem valde concentratum, aſt majori adhuc olei flavi copia combinatum deprehenditur. Hoc, ope infundibuli ab illo quidem ſeparavi, et acidum in oleo latens adjecto ſpiritu vini extrahere tentavi; ſed eodem momento, quo vini ſpiritus addebatur, oleum jam ſolutum erat, nec adjecta aqua ab illo ſeparari poterat.

EXP. LIV. Acidi concentrati (exp. 53.) ℥ ß æqualem partem ſpiritus vini optimi admiſcui. Maſſam digeſtam per 12 horas igne lampadis diſtillavi. Odor liquoris in excipulo oleo vini valde ſimilis erat. Adjecta aqua lactis fere faciem induebat; paulo poſt ſuperficiei innatabat oleum, reddita antiqua pelluciditate, ponder. ʒ iij, ſaporis aromatici, ſed oleo vini mitioris. Hoc itaque periculo novum accedit robur ei ſententiæ, acidum nempe quodvis, ope vini ſpiritus producere poſſe oleum tenuiſſimum

artificiale; noſtra vero in ſpecie naphtha virtutibus medicis quam maxime pollere cenſenda eſt, quæ penetrantiſſima eſt, et originis animalis.

EXP. LV. Oleum in vini ſpiritu ſolutum diſtillavi leniſſimo calore, quo tranſiit naphtha, addita aqua conſpicua. Hac operatione oleum e flavo fuſcum ſeſe ſeparavit, et ſuperam liquoris partem in retorta occupavit. Protracta diſtillatione tandem guttulas olei in naptha, in excipulo contenta, conſpexi; quo facto finem impoſui; et ob intimiorem acidi cum vini ſpiritu quam cum oleo combinationem, majorem horum volatilitatem, a fixiori oleo ſeparavi.

Superſunt adhuc pericula hujus acidi cum metallis inſtituenda: datur quoque facilior breviorque methodus, acidum illud concentratum obtinendi, ſi nempe ſapo (ſed ſine ſale communi factus) ſolutus, adjecto alumine deſtruitur, colatumque liquidum evaporatur, tuncque cum alumine calcinato forti igni exponitur. Quod hoc modo eduxi acidum illud quam maxime ſingulare edidit phænomenon, ut, ſolum, foliola quædam auri diſſolveret. De quorum vero omnium uberiori et accuratiore tractatione, ſi hæcce indigna haud videbuntur, proxima occaſione lubentiſſime ſum communicaturus. Dabam Helmſtadii d. 11 Nov. 1777.

VIII. *Account of a Woman who had the Small Pox during Pregnancy, and who seemed to have communicated the same Disease to the Fœtus. By* John Hunter, *Esq. F. R. S.*

Read January 17, 1780.

MR. GRANT'S ACCOUNT.

ON the 5th of December, 1776, Mrs. FORD had been seized with shivering and the other common symptoms of fever, to which were added great difficulty of breathing and a very hard cough. Mr. GRANT saw her on the 7th; and he took from her eight ounces of blood, and gave her a composition of the saline mixture with spermaceti and magnesia every six hours.

This had operated by the 8th two or three times very gently, when most of the complaints were relieved; but the cough still shaking her violently, bleeding seemed necessary to be repeated, more particularly as she looked upon herself to be in the sixth month of her pregnancy. The medicine was continued without the magnesia.

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In the evening (*viz.* the 8th) the ſmall pox appeared, which proved of a mild kind, and moderate in quantity. Its progreſs was rather ſlower than might have been expected; but the woman paſſed through the diſeaſe in great ſpirits, ſitting up the greateſt part of the day during the whole time, and taking only a paregoric at night, and, as occaſion required, a little magneſia: thus the ſymptoms were mitigated, and the cough at laſt became very little troubleſome.

On the 25th ſhe complained of a pain in her ſide. Eight ounces of blood were taken away. The next day ſhe was quite free from pain, and thought herſelf as well on the 27th as her particular ſituation would admit of; after which ſhe was not viſited by Mr. GRANT till the 31ſt, when ſhe was in labour.

Mr. WASTALL'S *letter on the ſame ſubject.*

December 30, 1776, I was ſent for to Mrs. FORD, a healthy woman, about twenty-two years of age, who was pregnant with her firſt child. She had come out of the country about three months before. Soon after her arrival in town ſhe was ſeized with the ſmall pox, and had been under the care of Meſſieurs HAWKINS and GRANT, who have favoured me with the particulars here annexed.

I called upon her in the afternoon; ſhe complained of violent griping pains in her bowels, darting down to the *pubes*. On examining I found the *os tincæ* a little dilated, with other ſymptoms of approaching labour. I ſent her an anodyne ſpermaceti emulſion, and deſired to be called if her pains increaſed. I was ſent for. The labour advanced very ſlowly; her pains were long and ſevere; ſhe was delivered of a dead child, with ſome difficulty.

Obſerving an eruption all over the body of the child, and ſeveral of the *puſtules* filled with matter, I examined them more particularly; and recollecting, that Dr. LEAKE, in his Introductory Lecture to the Practice of Midwifry, had obſerved, that it might be neceſſary to inquire, whether thoſe adults who are ſaid totally to eſcape the ſmall pox have not been previouſly affected with it in the womb, I ſent a note to Dr. LEAKE, and likewiſe to Dr. HUNTER, in hopes of aſcertaining a fact hitherto much doubted. Dr. LEAKE came the ſame evening, and ſaw the child. Dr. HUNTER came afterwards, with Mr. CRUICKSHANKS, and examined it; alſo Mr. JOHN HUNTER and Mr. FALCONER; who all concurred with me, that the eruption on the child was the ſmall pox. Dr. HUNTER thought the eruption ſo like the ſmall pox that he could hardly doubt; but ſaid, that in all other caſes of the ſame kind,

that he had met with, the child *in utero* had escaped the contagion.

From Mr. GRANT'S *notes.*

The eruption appeared on Mrs. FORD in the evening of the 8th of December, and she was delivered the 31st, that is, twenty-three days after the appearance of the eruptions.

Reflections by Mr. JOHN HUNTER.

The singularity of the above case, with all its circumstances, has inclined me to consider it with some attention.

There can be no doubt but that the mother had the small pox, and that the eruption began to appear on the 8th of December: also, that it went through its regular stages, and that on the 31st, *viz.* twenty-three days after the first appearance of the eruption, the woman was delivered of the child, who is the subject of this paper.

Secondly, The distance of time when she had the small pox before delivery, joined with the stage of the disease in the child when born, which probably was about the sixth or seventh day of the eruption, *viz.* about fifteen or sixteen days after the beginning of the eruption on the mother, perfectly agrees with the possibility of the infection's being caught from the mother.

Thirdly, The external appearance of the *pustules* in the child was perfectly that of the small pox, as must have appeared from the relation given in Mr. WASTALL'S letter.

letter. Moſt of the *puſtules* were diſtinct, but ſome were blended or united at their baſe. The face had the greateſt number; and theſe were in general the moſt indiſtinct. They were ſomewhat flattened with a dent in the middle[(a)].

So far were the leading circumſtances and external appearances in favour of their being the variolous eruption; but although theſe leading circumſtances and external appearances were incontrovertible, yet they were not an abſolute proof of this being the genuine ſmall pox; therefore I muſt be allowed to conſider this ſubject a little further, and ſee how far all the circumſtances correſpond or are ſimilar to the true ſmall pox. In the ſmall pox we have a previous fever, in place of which, in the preſent caſe, we have no information but that of the mother's having had the ſmall pox within ſuch a limited time as may favour the poſſibility of infection in the womb; yet we may preſume, that the child muſt have had conſiderable fever preceding ſuch an eruption, of whatſoever kind it was.

In the ſmall pox the eruption goes through pretty regular ſtages in its progreſs and declenſion, which circumſtances we know nothing of in the preſent caſe; but

(*a*) I endeavoured to take ſome matter upon the point of two lancets; but not having an opportunity of making an experiment myſelf, I gave them to two gentlemen, who, I imagine, were afraid of inoculating with them.

even

even this fever, the eruptions, and their progreſs, are not abſolutely proofs that the diſorder is the ſmall pox when it is caught in the common and natural way: and in proof of this aſſertion it may be obſerved, that practitioners every now and then are miſtaken.

It may be aſked, what is the true characteriſtic of the ſmall pox? That by which it differs from all other eruptions that we are acquainted with? The moſt certain character of the ſmall pox, that I know, is the formation of a ſlough, or a part becoming dead by the variolous inflammation; a circumſtance which hitherto, I believe, has not been taken notice of.

This was very evident in the arms of thoſe who were inoculated in the old way, where the wounds were conſiderable, and were dreſſed every day; which mode of treatment kept them from ſcabbing, by which means this proceſs was eaſily obſerved; but in the preſent method of inoculation it is hardly obſervable: the ſore being allowed to ſcab, the ſlough and ſcab unite and drop off together. The ſame indiſtinctneſs attends the eruptions on the ſkin; and in thoſe patients who die of, or die while in, the diſeaſe, where we have an opportunity of examining them while the part is diſtinct, this ſlough is very evident.

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This ſlough is the cauſe of the pitt after all is cicatrized; for it is a real loſs of ſubſtance of the ſurface of the *cutis:* and in proportion to this ſlough is the remaining depreſſion.

The chicken pox comes the neareſt in external appearance to the ſmall pox; but it does not commonly produce a ſlough.

As there is generally no loſs of ſubſtance in this caſe, there can be no pitt. But it ſometimes happens, although but rarely, that there is a pitt in conſequence of a chicken pock; then ulceration has taken place on the ſurface of the *cutis*, a common thing in ſores.

In the preſent caſe, beſides the leading circumſtances mentioned in the caſe of the mother, correſponding with the appearances on the child, and the external appearances themſelves, we have in the fulleſt ſenſe the third and real or principal character of the ſmall pox, *viz.* the ſlough in every *puſtule*; from all which, I think, we may conclude, that the child had caught the ſmall pox in the womb; or at leaſt a diſeaſe, the effects of which were ſimilar to no other known diſeaſe.

In opening the bodies of thoſe who had either died of, or died while under, the ſmall pox, I always examined carefully to ſee whether any internal cavity, ſuch as the *œſophagus*, *trachea*, ſtomach, inteſtines, *pleura*, *peritoneum*,

neum, &c. had eruptions upon them or not, and never finding any in any of thofe cavities, I began to fufpect, that either the fkin itfelf was the only part of the body fufceptible of fuch a *ftimulus*; or that the fkin was fubject to fome influence to which the other parts of the body were not fubject, and which made it alone fufceptible of the *variolous ftimulus*. If from the firft caufe, I then concluded it muft be an original principle in the animal œconomy. If from the fecond, I then fufpected, that external expofure was the caufe; and I was the more led into this idea, from finding that thefe eruptions often attack the mouth and throat, two expofed parts; add to which, that we generally find the eruptions moft on the expofed parts of the body, as the face, &c.

With thefe ideas in my mind, I thought I faw the moft favourable opportunity of clearing up this point. I therefore very attentively examined moft of the internal cavities of this child; fuch as the *peritoneum*, *pleura*, *trachea*, infide of the *œfophagus*, ftomach, inteftines, &c. but obferved nothing uncommon. I have already obferved, that in this child the face and extremities were the fulleft, fimilar to what happens in common; from all which I may be allowed to draw this conclufion, that the fkin is the principal part which is fufceptible of the

variolous

variolous *ſtimulus*, and is not affected by any external influence whatever.

The communication of the ſmall pox to the child in the womb may be ſuppoſed to happen in two ways; one by infection from the mother, as is ſuppoſed in the above caſe; the other by the mother's having abſorbed the ſmall pox matter from ſome other perſon, and the matter being carried to the child from the connection between the two, which we may ſuppoſe done with or without firſt affecting the mother.

Teſtimonies and opinions are various with reſpect to theſe two facts. BOERHAAVE ſeems to have been led by his experience to think that ſuch infection was not communicable: for we find that he attended a lady, who having, in the ſixth month of her pregnancy, had the confluent ſmall pox, brought forth at the regular period a child, who ſhewed not the leaſt veſtige of his mother's diſeaſe.

His commentator, however, VAN SWIETEN, ſupports a different opinion (ſee his Comment, vol. V.). He quotes a caſe from the Philoſophical Tranſactions, vol. XXVIII. N° 337. p. 165. of a woman, who, having juſt gone through a mild ſort of ſmall pox, was, by means of a ſtrong doſe of purging phyſic, thrown into a miſcarriage,

 and

and brought forth a dead female child, whofe whole body was covered with *variolous puftules* full of ripe matter; but this hiftory is founded only on the relation of a a midwife to a clergyman, and therefore not abfolutely to be depended upon as accurately ftated: however, it is more than probable, that there was a cafe as defcribed; and that there were really eruptions on the fkin of the child fimilar to the fmall pox.

VAN SWIETEN likewife mentions what MAURICEAU relates of himfelf. This author teftifies, that he had often heard his father and mother fay, that the latter, when big with him, and very near her time of delivery, had a painful attendance on one of her children, who died of the fmall pox on the feventh day of the eruption; and that on the day following the death of this child, MAURICEAU came into the world, bringing with him five or fix true *puftules* of the fmall pox.

It does not appear, however, from this recital, whether or not MAURICEAU paffed through life free from any pofterior infection; but admitting that this eruption of MAURICEAU's was truly the fmall pox, yet I fhould very much doubt his having caught it from the child who died of it: as it fhould feem that the *puftules* of MAURICEAU were of the fame date with thofe of the child who

died.

died. VAN SWIETEN appeals to a more recent cafe, which had been reported to him by perfons of great credit, and is recorded in the Phil. Tranf. vol. XLVI. p. 235.

" A woman, big with child, having herfelf long ago
" had the fmall pox, very affiduoufly nurfed a maid fer-
" vant during the whole procefs of this difeafe. At the
" proper time fhe brought forth a healthy female child,
" in whofe fkin Dr. WATSON afferted, that he difcovered
" evident marks of the fmall pox, which fhe muft have
" gone through in the womb; and the fame phyfician
" pronounced, that this child would be free from future
" infection. After four years her brother was inoculated;
" and Dr. WATSON obtained permiffion of the parents to
" try the fame experiment on the girl. The operation
" was performed on both children in the fame manner;
" and the *pus* ufed in both cafes was taken from the
" fame patient. The event, however, was different; for
" the boy had the regular eruption, and got well; but
" the girl's arm did not inflame nor fuppurate. On the
" tenth day from the infertion of the matter, fhe turned
" pale fuddenly, was languid for two days, and after-
" wards was very well. In the neighbourhood of the
" incifion there appeared a *puftule* like thofe *puftules* that
" we fometimes obferve in perfons who, having had the
" difeafe, attend patients ill of the fmall pox.

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In the epiſtles of T. BARTHOLINUS, cent. II. p. 682. there is the following hiſtory. "A poor woman, aged "thirty-eight years, pregnant, and now near the time of "delivery, was ſeized with the ſymptoms of the ſmall "pox, and had a very numerous eruption. In this ſtate "ſhe was delivered of a child, as full of *variolous puſtules* "as herſelf. The child died ſoon after birth; the mo-"ther three days afterwards." VAN SWIETEN infers, that the mother and the child were in this caſe infected at the ſame time; therefore, the child not infected by the mother.

Dr. MEAD aſſerts, that when a woman in the ſmall pox ſuffers an abortion, the *fœtus* is generally full of the contagion; but that this does not happen always. This variety, he ſays, depends on the ſtate of the mother's *puſtules* when the child is born; that is, whether they are or are not in a ſtate of purulence. Whence he has obſerved it ſometimes to happen, that on the ſecond day from the birth, or the third, or any day before the eighth, the diſeaſe caught from the mother ſhews itſelf in eruptions on the child.

Dr. MEAD here relates the hiſtory of a lady of quality, of which this is the ſubſtance. A lady, in the ſeventh month of her pregnancy, had the confluent ſmall pox, and on the eleventh day of the diſeaſe brought forth a ſon, having no ſigns of the diſeaſe on his body; and ſhe

died on the fourteenth day. The infant having lived four days, was feized with convulfions, and, the fmall pox appearing, died. The doctor infers from hence, that the fuppuration being in fome meafure compleated on the eleventh day, the mother's difeafe was communicated then to the *fœtus*, and made its appearance on the child after eight days.

If there be no abortion, Dr. MEAD pronounces, that the child will ever be free from the difeafe, unlefs the birth fhould happen before the maturation of the *puftules*. He brings a cafe to prove, that the *fœtus* in the womb may be infected by the contagion of which the mother does not partake. "A woman, who had long "before fuffered the fmall pox, nurfed her hufband, "under that difeafe, towards the end of her pregnancy; "and was brought to bed at the due time. The child "was dead, and covered all over with *variolous puftules*.

With refpect to the cafe quoted from MAURICEAU, it has been proved by Sir GEORGE BAKER (Med. Tranfact. vol. II. p. 275.) that Dr. MEAD drew a conclufion from it directly contrary to the author's meaning. The negative opinion appears evidently to be fupported by that hiftory.

Sir GEORGE BAKER mentions in the fame paper the cafe of two pregnant women who were inoculated at Hertford. They both had the fmall pox favourably, and

afterwards brought forth their children perfectly healthy at the ufual time. Both thefe children, at the age of three years, were inoculated with effect.

Sir GEORGE BAKER likewife mentions a cafe which fell under the obfervation of Dr. CLARKE of Epfom. "A wo-"man towards the end of her pregnancy had the fmall "pox, from which fhe narrowly efcaped. Five weeks "after the crifis fhe was delivered of an healthy female "child, who having numerous marks on her fkin was "judged by all who faw her to have undergone the fame "diftemper before her birth. However, at the end of "twelve months fhe had the fmall pox in a very fevere "manner. Both the mother and child were lately living "at Epfom."

Since then we fee that it is very probable, that the fmall pox may be caught from the mother when fhe is infected, it may be afked, why does not this happen oftener? In anfwer to this we may fuppofe, that this is not fo ready a way as when the child is expofed to catch it after the birth, as we find too that a difference can be produced after birth; *viz.* inoculation is a much readier way of catching it than what is called the natural way. It may likewife be faid, that many women who are with child, and have the fmall pox during pregnancy, do not recover; therefore both mother and child die before the difeafe can have time to produce eruptions upon the child.

child. Finally in many of thoſe caſes, where the mother recovers, there is ſometimes produced a miſcarriage, which alſo hinders the infection from taking place in the child. However, many women go through the whole diſeaſe, and the child ſhews no marks of the ſmall pox.

Thus have I ſtated facts relative to the preſent ſubject, with ſome of the beſt authorities on both ſides of the queſtion; and ſhall now leave the reader to form his own judgement.

IX. *Ett kort utdrag af en Journal, hållen på en reſa til och uti Kejſaredömet Japan, gjord af Doctor* Thunberg *åren* 1775 *och* 1776, *ſkrifvit til Herr* Joſeph Banks, *Præſes uti Royal Society, i* London [(a)].

Read Feb. 10, 1780.

NÄR min Herre, i början af innevarande år, under mit viſtande i London, behagade introducera mig uti åtſkillige ſällſkaper, der flere af Wetenſkaps Societetens reſpective Ledamöter voro tilſammans, gjordes mig åtſkillige frågor, angående det ſynnerliga landet Japan och deſs inbyggare, ſom jag då uti ſtörſta korthet hade den äran at beſvara; men för at vidare contentera ſå väl Societetens, ſom min Herres enſkilta nyfikenhet, adreſſerar jag til min herre detta mycket korta utdrag af den Journal, jag under mit 16 månaders viſtande i det aflägſna Japan hållit.

Det är min Herre förut bekant, at jag af Commiſſarierne öfver Hortus Medicus och några andre förnäme Herrar i Amſterdam blef utſkickad förſt til Cap de Bonne Eſperance och ſedan til Japan, at göra uti Hiſtoria natu-

(a) For a tranſlation of this paper ſee the Appendix.

rali

rali nya uptäkter, och at deraf öfverſända lökar och frön, ſamt lefvande träd och buſkar. Jag uppehöll mig try års tid på Cap de Bonne Eſperance, et ſtälle, ſom fournerade mig ganſka många nya, både Djur och Växter, och gick derifrån år 1775 til Batavia, hvarifrån jag kort derefter, nemligen den 21 Junii, affeglade til Japan på Holländſka Compagniets ſkjep Staveniſſe, i följe med et annat ſkjep, kalladt Blyenburg. Reſan fortſattes, ſom vanligt ſker, förbi Pulo Sapato, Chineſiſka landet och Formoſa, igenom ſundet der och Japanſka Golfen, til Nagaſaki hamn, ſom är den endaſte i hela riket der något ſkjep har frihet at inſegla.

Vi utſtodo flera ſvåra ſtormar, hvaraf det medföljande ſkjeppet Blyenburg blef ſå illa medfarit, at ſedan masterne blifvit förlorade, det ej kunde fortſätta reſan til Japan, utan nödgades löpa in til Canton, at der repareras, och ſedan gå tilbaka til Batavia.

Den 13 Auguſti fingo vi Japan i ögnaſigte, och kommo in uti Nagaſaki hamn dagen der efter. Vi inſeglade uti hamnen med ganſka många uphiſſade flaggor, och låſſade våre Canoner vid det ſå kallade Papenberget, vid Kejſarens och Kejſarinnans vagter och före Nagaſaki ſtad. Under inſeglandet kommo om bord tvänne Öfver-Banjoſer, åtſkillige Tolkar och under officerare, ſamt några Committerade ifrån Holländſka Factoriet.

Öfver

Öfver Banjoſerne, ſom kunna jämnliknas med Mandarinerne i China, gingo ſtraxt at ſitta på et ſtälle, ſom var til den ändan tilredt, mitt på Skjepsdäcket, hvilket ſäte de ſedan altid nyttjade, ſå ofta de viſtades om bord. Deſſe Officerare, ſom växlas om, äro altid om bord de dagar, då ſkjeppet loſſas eller laſtas, och ingen ting får föras hvarken til eller ifrån ſkjeppet, under all den tid det ligger i hamnen, utan at de äro tilſtädes. De äga upſigt öfver alt det, ſom går ut eller in i ſkjeppet: för deras ögon viſiteras både folck och gods: framför dem mönſtras hvarje gång alt ſkjepsfolcket: de förläna och underteckna alla paſsporter, och göra dageligen rapport af alt, hvad ſom förefaller, til Gouverneuren i Nagaſaki.

Det förſta, ſom förekom oſs underligt och ovanligt, var den ſtarka viſitation, ſom hvar och en perſon, och alt det gods, ſom fördes til eller ifrån ſkjeppet, måſte undergå. Denna viſitation förrättades icke allenaſt ganſka noggrant, utan den ſkjedde dubbelt, nämligen både på ſkjeppet och i land. Sängkläderne upſprättades, och ſjelfva fjädrarne i dem ranſakades: kiſtor utpackades ända til botn, och ſjelfva plankorne i dem klappades på alla ſidor, til at uptäcka, om in uti dem voro några hemliga gjömmor: ſmörpottor och confitur-krukor rördes inuti med en jernſpits: uti oſtar ſkars förſt et ſtort hål, och ſedan ſtacks en knif in åt alla ſidor: ja de miſstrodde ſjelfva äggen och ſlogo

äfven deraf åtskillige i stycken. Hvarje person, som går ifrån eller til skjeppet, visiteras äfven lika skarpt. Visitatorn stryker med sina händer längs ryggen, sidorne och magen, ända ned til knän, så at ingen ting på lifvet kan gjömas, utan at han uptäcker det. Denne visitation måste nu alla, utan åtskilnad, undergå, ända ifrån Chefen intil den sämsta matros, enligt de ordres, som år 1775 ankommo ifrån Kejserliga Hofvet.

Tilförene var denna visitation icke allenast mindre sträng, utan Chefen och skjeps Capitenerne voro aldeles frie ifrån den samma, och Capitenerne hade då tilfälle, at lurendräga in så mycket gods, de ville, til hvilken ända de voro klädde i en ganska stor och vid öfverråck, med twänne stora säckar eller fickor på inre sidorne, som fyldes med contraband-varor, när de gingo til eller ifrån land, hvilcket skjedde gemenligen trenne gånger om dagen.

Orsaken til denna stränga visitationen äro Europæerne sjelfve, som ifrån den ena tiden til den andra, med sine lurendrägerier retat Japanska Regeringen, til at mer och mer inskränka deras frihet; och då den samma länge nog märkt, at ju nogare de visiterades, ju finare de öfvade sine bedrägerier, så har den omsider tagit deremot sådane mått och steg, at nu mera för den aldrafintesigaste icke en enda utväg mer är öfrig til oloflig handels drifvande. För några år sedan hade en skjeps under-

under-officerare in uti fjelfva byxorne fördöljt en fogel, i tanka at fälja den famma, då han kommit i land; men under fjelfva vifitationen, til olycka, begynte foglen at fjunga och bedrägeriet uptäcktes: fedan den tiden vifiteras utanpå hos Matroferne och fämre folck byxorne, och fjelfva håret på Slafvarne.

Japonefen är til färgen gulagtig, ehuru man ibland finner någre, i fynnerhet Fruntimren, fom äro mycket hvita. De fkiljas ifrån alle andre nationer, undantagne Chinefen och Tartaren, med fine djupe och aflånge ögon famt höga ögonbryner. Näfan är ej platt, ehuru hon är något tjockare och kortare, än på en Europé. Håret är fvart, och upfättes på enahanda vis, få at en främling här icke utan förfta förundran, anfkådar et almänt mode, fom uti hår-frifure och klädning, hos en hel nation, är oförändradt, ända ifrån Kejfarens Thron intil den lägfta koja.

Männerne upfkicka fit hår på helt annatt fätt, än andra könet: hela förhufvudet och midden ända til nacken afrakas aldeles, och hålles äfven få frit för hår, fom hakan ifrån fkjägg; det öfriga, fom är vid tinningarne och i nacken, upftrykes och bindes öfver hjeffan tilhopa med en hvit tråd; utom denna bindningen lämnas håret få långt fom en finger, och klippes der tvärtaf, ombindes

med resten af förra tråden, och böjes emot hufvudet, lika som en krokug finger.

Andra könet behåller alt sit hår, utan at deraf afraka något. Håret upstrykes emot kullen och viras der tilhopa i en knut, samt utdrages åt sidorne i tvänne breda vingar: bakom knuten sättes en kam, och igenom knuten åtskillige långa zirater utaf olika skapnad och ämne.

Medici och Präster, som gemenligen låta raka hela hufvudet, och barn, som ej ännu hunnit til manlig ålder, äro de endaste, som göra i detta modet något undantag, til at skilja sig ifrån de öfriga af nationen.

Hela nationen har enahanda och sin egen särskilda klädedrägt, som ifrån urminnes tider varit oförändrad. Den består uti en eller flere Nattråckar, som bindas til om lifvet med et bälte. Fruntimren hafva gemenligen desse längre, och ofta fläpande efter sig. Desse nattråckar äro om sommaren ganska tunna, och om vintren upstoppade med tjock silke- eller bomuls-wadd. De förnäme nyttja härtil silke, och de fattige bomuls tyger. Fruntimren nyttja deraf gemenligen et större antal, och äga dem prägtigare, ofta med inväfne guld- och silfver blomor.

Nattråcken är vid bröstet något öpen, och har mycket vida armar, hvars öpning framtil är til hälften ihopsydd, och formerar en säck, innom hvilken de kunna indraga händerne,

händerne, och i hvilken de kunna bevara papper och andre saker, lika som i en ficka. Männerne hafva utom dess en mantel eller half nattråck, som räcker til höften, af ganska tunt, svart tyg, och utan på nattråcken långa byxor, som mycket likna en fruntimmers kjortel, öpen ganska långt ned på bägge sidor. Med desse tvänne sednare skiljes bättre folck ifrån den sämsta pöbelen. Månge nyttja underbyxor utaf linne, men benen äro altid nakne. De bruka aldrig skor, utan endast tofflor af halm flätade, försedde med en bögel och et band, som stickes emellan stortån och den nästa tån, til at hålla dem fast. Endast i vintren bruka de såckor af linne på fötterne. När det är mycket orent och regnväder, nyttja de höga träskor. Hufvudet betäckes aldrig, om icke de äro på resor, då de nyttja en conisk hatt af halm; utan de bruka solskärmar (parasols) at skydda hufvudet för stark solhetta och regn.

Uti bältet, som om lifvet fasthåller nattråcken, infättes sabelen, solfjädren och tobakspipan. Sabelen instickes på vänstra sidan, så at skarpa äggen altid vetter upåt. De, som äro uti någon public tjänst, draga altid tvänne sablar, hvaraf den ena altid är längre.

Husen byggas af träftolpar, som på någon distance inhuggas i hvarandra, nästan som på korsverkshus; emellan desse flätas sönderklyfde bambokäppar, och på ymse

sidor

sidor af dem bestrykes lera och hvitlimmas, så at de likna et stenhus. Våningarne äro ofta tvänne, hvaraf den öfversta är lågare, och nyttjas ei så almänt til boning. Taken äro merendels täkte med stora och tunga, fastän vackra och väl gjorda pannor af blå lera. Golfvet, som altid är uphöjdt en aln ifrån jorden, beläggeș med bräder och ofvan på dem aflånga mattor, flätade och fylde med halm, utaf try eller fyra fingers tjocklek. Hela huset utgör et enda stort rum, som sedan kan afdelas i så många smärre kamrar, som de behaga, med rammar, som insättas i dertil inrättade träbjälkar, som så väl uptil under taket, som nedtil på golfvet, blifvit inlagde. Sådane rammar äro gemenligen öfverdragne med tjockt måladt papper, och likna våre tapeter. Fönstren bestå äfven af rammar, afdelte i små rutor, och påklistradt hvitt, ganska tunt papper.

Man finner aldrig några meubler uti Japonesens hus, hvarken speglar eller väggur, skåp eller bureauer, stolar eller bord, kakelugnar eller spisar, soffor eller sängar. Deras sed är, at sitta ned på de mjuka och snygga golfmattorne, läggande sina fötter under kroppen, så at deras egna hälar blifva deras stol. Under måltiderne framsättes hvarje rätt särskild, äfven på desse mattor, dock på et litet, knapt try eller fyra tums högt bord. Speglar, som de likväl ej upsätta i huset til någon prydnad, göra de

af

af en componerad metall, samt nyttja dem endast vid hårets uplägning.

Spisar och kakelugnar äro hos dem aldeles obekante, fastän den stränga blåsten och kölden tvingar dem, til at värma sina hus ifrån Novembris til Martii månad; men i dess ställe nyttja de stora koppargrytor med breda flata bräddar och fötter, hvilke insättas i rummen, på sjelfva golfvets halmmattor. Desse grytor beklädas in uti med lerbruk, och fyllas til en del med ren aska, hvarpå väl utbrända furukol upstapplas, som i brand stukne upvärma rumet, utan at osa.

Tobakens bruk har utan tvifvel hos Japoneserne blifvit först bekant igenom Portugiserne; men fastän bägge könen här röka och äfven ungt folck, så skjer dock sådant mycket sparsamt. När de vela fägna en ankommen främande, så är gemenligen tobakspipan och en kopp thévatn det första, som bjudes honom. Pipan består af et kortare eller längre väl gjordt träskaft med munstycke och hufvud, utaf mässing eller hvit koppar. Sjelfva hufvudet är ej större, än at det kan ryma en stor ärt, och sjelfva tobaken, som är fulkomligen torr, karfvas så fin, som hufvudhår, utaf en fingers längd: den måste altså vridas ihop liksom til en pill, för at kunna stoppas i piphufvudet. När den blifvit stucken i brand, är pipan

lätteligen.

lätteligen och med några få drag utrökt. På detta sätt röka de flere pipor å rad, samt utblåsa röken långsamt igenom näsbororne.

Solfjädren nyttjas utaf bägge könen, at dermed i hettan svalka sig, och är derföre altid både innom och utom hus deras trogna följeslagare.

Hela nationen är fallen för snygghet och renhet; man finner derföre uti hvarje hus, och uti alla herberg för resande en badstuga, der de hvarje dag, utan försummelse, tvätta sig.

Sällan eller aldrig får man se någon mansperson, som icke har sina kläder teknade med sit sigils märke, hvilket med samma färg, hvarmed tyget blifvit trykt eller färgadt, på små dertil på armar och rygg hvite lämnade runde fläckar blifvit teknat, och gör, at ägaren altid igenkänner sin klädning, skjönt den funnos lagd ibland hundrade andra.

Hörsamhet och lydnad emot föräldrar samt ödmjukhet emot förmän i agt tages här på det alranogaste. Det är nöjsamt at se, med hvad respect, som en förnäm och en Ämbetsman bemötes. De stadna icke allenast, om de möta en sådan, eller om han råkas i något hus, hålla sig på längre afstånd ifrån honom, utan böja äfven sine hufvuden ända ned til jorden. Vänliga äro de altid emot hvarandra,

hvarandra, i hälsningar, tal och åtbörder. Barnen vänjas härvid ifrån första barndomen, och föräldrarne föregå dem med sit exempel. Aga och straff äro sällsynte i landet, men lagar och straff ganska stränga. Människjor äro väl af naturen på alle orter benägne til ondt, men knapt gifves något land, der mindre förbrytelser skje emot landets lagar.

Uti deras namn är någon ting, som förtjänar upmärksamhet. Stam-namnet nyttjas aldrig i dageligt tal, utan endast, då de undertekna någon skrift. Med förnamnet åter nämnas och ropas de i dagligt tal, och det samma förändras efter deras olika åldrar och efter olika syslor, som de med tiden bekläda, så at de i sin lifstid ibland ägt fem eller sex särskilde förnamn, fastän familie namnet altid varit oförändradt.

Sin ålder räkna de altid jämnt med det året, på hvilket de varit födde, antingen det skjett i början eller slutet: således räknas et barn altid årgammalt, så snart nyårsdagen infaller, änskjönt det kommit til verlden endast några dagar förut.

Handel och slögder äro hos Japanska nationen i flor, fast icke i den grad, som i Europa; ty deras behof äro ej så många: men Åkerbruket är hos dem uti första värde, så at man här ser jordytan öfver alt, och de mäste bergen

ända up til topparne upbrukade. Utrikes handel drifva de endast med Holländska och Chinesiska Compagnierne. Emot Koppar och rå Campher, som Holländaren utförer, återtaga de Puderfocker, Modernäglickor, Sappanträ, Elfenben, tenn, bly, skilpadskal, Chitser, Taffecelastyger och någre andre småsaker.

Som Holländska Compagniet i Japan aldeles icke betalar någon tull för ut-eller in-gående varor, så sändas i det stället årligen skänker til Hofvet, som bestå i kläden, Chitser, Succotas, bomulstyger och stundom några rariteter. På denna resa, som förrättas endast af trenne Europeer och vid pass tvåhundrade Japoneser, hade jag det nöjet, at följa Ambassadeuren til den längst bort i landet belägna Hufvudstaden Jedo.

Den 4 Martii 1776 afreste vi ifrån vår lilla ö Dezima och Nagasaki stad igenom Kokora til Simonoseki, der vi den 12 gingo om bord på et för oss tilredt stort fartyg, hvarmed vi innom skärs afseglade til Fiogo. Resan fortsattes vidare öfver land til Osacca, en af de förnämsta handelsstäder i Riket, der vi tilbragte den 8 och 9 Aprilis. Innom en dags resa ankommo vi den 10 derpå följande til Miaco, Andeliga Kejsarens Hufvudstad. Här förtöfvade vi tvänne dagar, och påskyndade vår resa til Hufvudstaden Jedo, dit vi lyckeligen anlände den 1 Maji.

Resan til lands skjer altid uti Norimons, en sort af Palanquins, som äro tilslutne och försedde med fönster, samt bäras utaf flere män. Skänkerne så väl som dageliga Provianten bäras äfven af folck uti kistor, och några få saker föras på packhästar. Officerare och Tolkar ledsaga ofs, och beförja om all ting, så att vi kunna göra en nöjsam och obekymrad resa.

Den 18 Maji var den dag, då vi hade hos Cubo, eller verldsliga Kejsaren, hos Kronprinsen och de tolf Riksens Råd vår audience; dagen derpå hos Tempelherrarne, som föra styret öfver kyrkorne, och Stadens Gouverneurer med flere. Den 23 påföljande hade vi vår afskjeds audience, afreste från Jedo den 26 Maji, och ankommo til Miaco den 7 Junii. Nu på återresan hade vi här den 8 Junii audience hos Kejsarens Ståthållare, som äfven får några skänker, efter vi aldeles icke få aflägga något besök hos Dairi. Den 11 fingo vi tilstånd, at vandra uti och ikring Staden, at bese deras kyrkor och större hus; men afreste emot aftonen derifrån til Osacca. Denna vackra och för sin handel väl belägna stad fingo vi frihet at genomvandra den 13, och at bese dess kyrkor, comœdier, andre märkvärdigheter, och i synnerhet huru kopparen smältes, som endast skjer i denna staden, och ei på något annat ställe mer i Riket. Hos Stadens

Gouverneurer hade vi den 14 audience, och fortſatte ſedan vidare vår reſa til Fiogo. Efter vanligheten gingo vi här på et ſtort fartyg den 18 om Bord, at ſjövägen afgå til Simonoſeki och Kokota, dit vi ankommo den 23. Sedan blefvo vi uti Norimons burne til Nagaſaki och vårt lilla factorie på Öen Dezima, hvareſt vi ſlutade den ſiſta Junii vår Ambaſſade, efter 118 dagars reſa.

X. *Account of an extraordinary Appearance in a Mist. By Mr.* William Cockin; *communicated by* Joseph Banks, *Esq. P. R. S.*

Read Feb. 20, 1780.

Lancaster, Sept. 2, 1777.

JANUARY 13, 1768, betwixt nine and ten in the morning, being on an eminence that overlooked some low meadow ground, I observed, in a direction opposite to that of the sun, which shone very bright, and in a mist which covered the said inclosures, an unusual meteor, which, without attempting to name it, I shall describe as well as I can by the help of the following figure.

At about the distance of half a mile, and incurvated towards each other, like the lower ends of the common rainbow, there appeared in the mist two places of a peculiar brightness as represented at AA. They seemed (as is common) to rest upon the ground, were continued as high as the mist, and in breadth, perhaps, near half as much more as that of the iris. In the middle, between these two places, on the same horizontal line, was a coloured

loured appearance like *dcb*, *a*, *bcd*, whose base could not at most subtend an angle of above ten or twelve degrees, and whose interior parts were thus variegated. The center *a* was dark and irregularly terminated, as if made by the shadow of some object not bigger than an ordinary sheaf of corn. Next this center was a curved space *bb*, of a yellowish flame colour. To this succeeded another curved space of nearly the same dark cast as the center, seemingly tinged with a faint hue of green, and very evenly bounded on each side, as is shewn at *cc*. After these came on the terminating ring, which was coloured very much in the manner of the common rainbow, except that the tints were not quite so vivid (as if owing to the effect of a yellowish tinge, which seemingly entered into the composition of all the colours) nor their boundaries so well defined. The center of the image appeared to be exactly in the line of aspect (as it is called) or one conceived to be drawn from the sun through the eye of the spectator; and it may be observed from the figure, that these curve spaces were not segments of perfect circles, but formed like the ends of concentric ellipses, whose transverse axises were perpendicular to the horizon.

To the above description of the image it may be necessary to add the following particulars which attended it.

The

The mist was very thick near the surface of the meadows, though rarer upwards, and chiefly, if not solely, on the side of the hill opposite to the sun. The place where I stood was just on its confines; and I found, as I advanced into it, that the object became gradually fainter and fainter. As the sun dispersed the vapour, the appearance faded proportionably; and about half an hour after I first saw it, it was scarcely visible. The evening before was wet; but the drops on the hedges were congealed by frost. Where the sun shone the bushes were each invested with a mist, as if owing to the vapours exhaled from them by the sun's warmth; and, on a nearer inspection (for there was something singular in this appearance), I was rather surprized to find, I could clearly discern the little humid particles which occasioned it, and which were floating around the bushes at about half an inch distance from one another.

Such were the most material circumstances of this beautiful and singular appearance. Singular no doubt it is, as we have only two instances of a like kind mentioned in Dr. PRIESTLEY's History of Light and Colours. The first is given by M. BOUGUER as seen upon the Andes[(a)];

(a) This is described as seen in a cloud consisting of frozen particles, and at about thirty paces distance. All the parts of the observer were clearly shadowed out, as legs, arms, and head, about which last parts the coloured circles were formed. It is farther noted, that the intervals between the circles continued equal, though their diameters were constantly changing.

and

and the other by Dr. MACFAIT as seen in Scotland(b). A third, however, may be met with as observed at Pambamarca(c), in ULLOA's Voyage to South America.

It is the pleasure of philosophy to attempt something by way of solution concerning every extraordinary fact which falls under its cognizance: and though it be not always so happy as to produce satisfaction, it may at least succeed in the way of amusement. Under the influence of these notions, let us see what offers respecting the philosophy of the curious appearance before us.

With regard to the elliptical form of the curve spaces, as it cannot be accounted for from refraction, I apprehend it is owing to the oblong figure of the observer's shadow, which is very evidently the dark part in the middle, and to which the coloured marginal rings are in some sort obliged to conform. The bright places AA correspond to an appearance once observed by Dr. SMITH(d), and which he very plausibly attributes to a confused mixture of the principal reflected beams

(b) This was seen in an extremely thick fog or mist. The interval corresponding to *bb* was observed to consist of colours similar to, though fainter than, those of *dd*.

(c) The apparent distance is here placed at about ten toises. The vapours are said to be of a tenuity cognizable by the sight. The gradual change of the diameter of the rings is mentioned; as also, that they appeared at first to be of an elliptical figure.

(d) See Art. 502. of the remarks at the end of his Optics.

that exhibit the ordinary bow. This muſt be owing to ſomething peculiar in the miſt, as to the form, ſize, &c. of its particles or globules; eaſier to conceive than explain. In the valuable Treatiſe of Optics by the laſt named gentleman, there is an account of Dr. PEMBERTON's theory for the ſlender rings of colours, which are ſometimes ſeen within the rainbow, which Dr. LANGWITH firſt deſcribed in the Philoſophical Tranſactions[(e)], and from which ſome idea may be formed of the cauſe of the coloured part of the image. It is in ſubſtance this[(f)]. If the drops of rain, &c. which the ſun ſhines upon be exceedingly ſmall, from the irregular reflection of all ſurfaces, and the fits of eaſy tranſmiſſion, which the diſſipated rays may undergo in their paſſage through thoſe little globules, there may naturally be formed other coloured arches within the common bow for a number of ſucceſſions. Hence, with regard to the inſtance in queſtion, ſince its rings were ſo very ſmall in diameter, it appears, that on ſome account or other the refracted, coloured, and diſſipated rays alluded to, have in their return to the eye nearly made the ſmalleſt angles poſſible with the lines of incidence.

(*e*) N° 375.
(*f*) See Articles 506, 507.

After all, this is only to be confidered as conjecture. Though it does not want analogy, we have not had it yet properly afcertained by experiment: and hence it is not without reafon, that Dr. PRIESTLEY confiders the additional rows of colours within the common rainbow obferved by Dr. LANGWITH, and thofe of the fpecies of image here defcribed, as one of the *defiderata* of optics.

J. BASIRE SC.

XI. *Memoria ſopra il Veleno Americano detto Ticunas. By the Abbé* Fontana, *Director of the Cabinet of Natural Hiſtory belonging to his Royal Highneſs the Grand Duke of* Tuſcany; *communicated by* John Paradiſe, *Eſq. F. R. S.*[a]

Read February 17, 1780.

LE eſperienze, che ho fatto a Parigi fino da due anni ſono ſopra il veleno della vipera, che fanno il ſeguito di molte altre pubblicate in Italia 10 anni prima ſul medeſimo ſoggetto; mi hanno meſſo in ſtato di pronunciar con ſicurezza ſopra la natura, e ſopra le proprietà di quel veleno.

Gli effetti inaſpettati, e importanti, che ho oſſervato applicando ai corpi viventi il veleno di quell' animale, mi hanno preſentate delle verità nuove per la fiſica animale; e queſte nuove verità mi hanno condotto per gradi a dubitare di qualche teoria medica non abbaſtanza provata, o troppo generalizzata dalle perſone dell' arte.

Fin d' allora ho deſiderato di portar le mie ricerche ſopra qualche altro veleno, e ſe foſſe ſtato poſſibile avrei

(a) For an Engliſh tranſlation of this paper ſee the Appendix.

desiderato di esaminar qualche veleno vegetabile de piu attivi. Mi figuravo, che i veleni animali, fossero comme è il veleno della vipera, per esempio, che applicato ad una ferita si diffonde ben si nel corpo dell' animale, ma non viene aumentato per questo, come è aumentato per contrario il veleno, che eccita il vajolo, o la rabbia del cane; mi figuravo dissi, che quei veleni avessero molta analogia fra loro, e che agissero nella medesima maniera; e sopra le stesse parti dell' animale.

Per l'opposto non osavo congetturar nulla sopra l'azione dei veleni végétabili, che non avevo ancor esaminati, e nulla mi pareva di potere avanzar con sicurezza sopra l'azione di essi, anche dopo la lettura de principali scrittori di quei veleni. La lor maniera di sperimentare era molto diversa da quella, che io avevo tenuta nell' esaminare il veleno della vipera, e le loro illazioni mi parevano troppo vaghe e incerte. Arrivato a Londra potei facilmente sodisfare ai miei desideri su di questo. Il Dr. HEBERDEN, celebre medico in Londra, e membro della Società Reale; mi ha procurato un gran numero di freccie Americane ben conservate, e ben coperte di veleno, e di piu ha avuta la gentilezza di farmi avere una buona quantità di veleno, che era chiuso, e sigillato dentro un vaso di terra, il quale aveva per di fuori una custodia di latta. Nella custodia di latta vi era un foglio, in cui si

leggevano le ſequenti parole: *Indian Poiſon brought from the Banks of the River of Amazons, by Don* PEDRO MALDONADO; *it is one of the ſorts mentioned in the Phil. Tranſ. vol. XLVII. n.* 12. Nel volume citato delle Tranſazioni vi ſi parla di due veleni poco differenti nella loro attivita. L'uno detto Lama, e l'altro Ticunas. Il veleno del vaſo di terra da me adoprato è il Ticunas Il veleno delle Freccie non ſi ſa bene a qual dei due appartenga, ma per eſperienza l'ho trovato della medeſima forza del Ticunas; onde non credo punto importante di diſtinguer l'una ſorta dall' altra.

Molte coſe ſi ſono ſcritte dell' attività di queſti veleni Americani dagli autori, talchè ho creduto bene di dover cominciar le mie eſperienze per gradi, prendendo tutte le precauzioni poſſibili. Il ſemplice odorarlo ſi crede nocivo, all' aprir ſolo il vaſo, e a'l ſolo diffonderſi di qualche molecola per l'aria ſi teme di qualche grave male, e fino la morte: coſi almeno ſi legge ne migliori autori.

Cominciai adunque ſubito aperto il vaſo del veleno di far reſpirar di quell' aria ad un tenero piccione, e lo tenni col capo dentro il vaſo per piu minuti. Lo levai di là che ſtava coſi bene che prima. Staccai con uno ſcalpello molti pezzi del veleno, perchè ſi eccitaſſe un poco di polvere nel vaſo, e allora vi immerſi di nuovo il capo del

Piccione,

Piccione, che non soffri nulla ne anco in questa seconda esperienza.

Da questo momento non ebbi piu alcuna difficoltà di espor me medesimo a quel vapore, e di sentirne l'odore, che mi parve nauseoso, e ingrato. Molte particelle mi entraronno coll' aria in bocca, e trovai che avevano un sapore simile in qualche cosa alla liquirizia. L'odore adunque di questo veleno a secco e affatto innocente, e innocenti sono le molecole, che entrano coll' aria nella bocca, o nel naso, e che vanno al polmone.

Ma dove par che piu si tema questo veleno, benchè ancora esterno, e allora che si riduce in vapori bruciato su' i carboni, e ridotto in fumo, o che e bollito lungamente, e inalzato in vapori densi. Nell' uno, e nell' altro modo ho voluto provarlo. Ho gettato piu pozzetti del veleno secco sopra i carboni accesi, ed ho fatto che il Piccione respirasse quel fumo, tenendolo colla testa nel mezzo di esso; il Piccione non ha mai dato segno di patir nulla. Ho fatto di piu: ho fatto entrare quel fumo in un tubo di vetro alto 6 pollici, e largo quattro. Il tubo era tutto ripieno di un fumo denso, e bianco, vi ho introdotto il solito Piccione, che non ha mostrato di soffrir di piu che se l'avessi introdotto nel fumo di zucchero brucciato. Son passato in sequito a farne bollire una buona quantità dentro un vaso di terra. Ho esposto ai vapori di esso

esso il Piccione; ve lo ho esposto quando già il veleno cominciava, a divenir consistente; ve lo ho esposto quando già fatto anco piu sodo cominciava a bruciare ai lati del vaso, e a ridursi tutto in vapori densissimi, e in carbone; l'animale non soffri nulla a nessuna di queste pruove; e allora fu che non ebbi neppur' io alcuna difficoltà di odorarlo, e di espormi a quei fumi. L'odore del veleno secco che brucia su i carboni è molto disgustoso, e sente l'odore di sterco bruciato.

Ne deduco da tutte queste esperienze, che i vapori, o fumi del veleno Americano odorati, o respirati sono innocenti. M. DE LA CONDAMINE è stato certamente ingannato, quando ha scritto, che questo veleno si prepara da donne condannate a morte, e che allora si conosce, che è arrivato alla sua perfezione, quando i vapori, che manda nel bollirlo, uccidono la persona, che e obligata ad esser presente.

Questo veleno si scioglie facilmente e bene coll' acqua anche fredda, come ancora con gli acidi minerali, e vegetabili. Nell' olio di vitriolo però si scioglie molto piu tardi, che negli altri acidi, e diventa nero, come l'inchiostro, che non succede con nessuno degli altri acidi. Non fa alcuna effervescenza nè cogli acidi, nè con gli alcalini, e non altera punto il latte, nè lo tinge, se non che del suo color naturale. Non altera il succo di radici, nè in roseo,

nè

nè in verde; ed esaminato col microscopio nulla si vede di regolare, e di salino, ma bensì par fatto in gran parte di corpicciuoli minimi irregolari rotondastri a somiglianza dei succhi vegetabili. Si disecca, senza screpolare, a differenza del veleno della vipera, e messo sulla lingua ha un sapore estremamente amaro.

Da tutto questo ne deduco, che non è nè acido, nè alcalino, e che non è composto di sali visibili, nè anco al microscopio.

Non tanto la curiosità quanto l'ordine, che mi ero prefisso nel far le mie esperienze, mi portarono ad esaminare, se questo veleno era micidiale applicato immediatamente sopra gli occhi, o se vi eccitava qualche malattia, o irritazione. Avevo già prima trovato, che il veleno della vipera era affatto innocente messo sugli occhi, come lo è per la bocca, e per lo stomaco; onde ero curioso di vedere i rapporti fra questi due veleni si attivi, e si diversi nella loro origine.

Cominciai adunque a metterne una picciola quantità sciolta nell' acqua, sopra l'occhio d'un porchetto d'India. L'animale non mostrò di soffrir nulla nè allora, nè poi, e l'occhio non s'i infiammo punto. Ripetei l'esperienze dopo due ore sopra tutti due gli occhi del medesimo animale, e vi applicai una maggior quantitá di veleno; il porchetto d'India non soffri il piu picciolo incomodo, e gli occhi si mantennero

mantennero nel loro ſtato naturale. Rinnuovai l'eſperienza ſopra gli occhi di due altri porchi d'India col medeſimo eſito; e il medeſimo eſito ebbero tutte le eſperienze, che feci in ſeguito ſopra gli occhi di molti altri animali, e ſpecialmente ſopra gli occhi dei cunigli. Non potei mai oſſervare alcuna alterazione de loro occhi, o che quel veleno gli incomodaſſe di piu di quel che avrebbe fatto l'acqua, ſe gli aveſſi bagnati con eſſa; onde credo di poter concludere, che il veleno Americano non è punto veleno meſſo ſugli occhi, e che non ha alcuna azione ſopra di eſſi.

Ma ſarà egli innocente preſo per bocca, e inghiottito? M. DE LA CONDAMINE, e tutti gli altri, che hanno par lato di quel veleno lo credono affatto innocente preſo per bocca e tale è l'opinione comune di tutti gli Americani. La ragione che lo ha fatto creder tale ſi é, che ſi poſſono mangiare impunemente gli animali uccisi con quel veleno o per meglio dire colle freccie avvelenate. Queſta ragione è piu ſpecioſa che convincente, perchè potrebbe eſſer veleno introdotto nel ſangue anche in piccoliſſima quantità, e non eſſerlo, ſe non che in una maggior doſe quando è preſo per bocca.

Ecco le eſperienze che ho fatto, le quali ſervono ancora a renderci cauti prima di pronunciare anche dopo che ſi è conſultata l' eſperienza medeſima.

 Feci

Feci inghiottire due grani di veleno sciolto nell' acqua ad un piccolo coniglio e la sforzai in seguito a bere un cucchiajo da Tè di acqua per dilavar la bocca, e per scendere tutto il veleno nello stomaco. Questo animale non mostrò di soffrir nulla, ne allora, nè dopo.

Feci bere ad un altro piccolo cuniglio, come sopra, trè grani di veleno, e non soffri nulla, come il primo. Ad un altro piccolo cuniglio feci bere quattro grani di veleno, e non ebbe nulla neppur' esso. Le medesime esperienze feci sopra trè piccoli cunigli, al terzo dei quali detti 6 grani di veleno, e non ebbe nulla, come tutti gli altri.

Credevo, che queste esperienze potessero bastare, per assicurarmi, che il veleno Americano è innocente preso per bocca, come lo è il veleno della vipera, ma mi sarei ingannato. Ebbi la curiosità di provarlo sopra un piccolo piccione; gli detti a bere 6 grani di veleno, e mori in meno di 20 minuti. Replicai l'esperienza sopra due altri piccioni, e morirono tutti due dentro 30 minuti.

Queste ultime esperienze, che par che contradicano le prime, mi hanno obbligato di ripeterne molte altre di nuovo sopra i conigli, e sopra i porchi d'India. Detti adunque a bere cinque grani di veleno ad un piccolo porchetto d'India, e lo trovai morto dopo 25 minuti. Feci bere circa 8 grani di veleno ad un altro piccolo cuniglio; e questo dopo 30 minuti pareva che non avesse nulla:

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ma dopo altri 30, cominciò a reggersi male su i piedi, dope altri 4 cadde come morto, e dopo altri quattro era morto affatto. Ad altri due piccoli conigli, e ad altri due porchetti d' India feci bere circa 9 grani di veleno. Un cuniglio mori in meno di 45 minuti, e in 20 minuti erano morti i due porchetti d'India. Questi risultato mi fecero credere, che una piu gran dose di veleno poteva produrre piu sicuramente la morte, e che l'istessa quantità di veleno produceva degli effetti diversi nei medessimi animali, secondo lo stato nel quale si trovava loro ventricolo. Avevo in generale osservato nel far le esperienze sudette, che gli animali morivano piu difficilmente quando avevano lo stomaco piu pieno, o non soffrivano nulla inghiottendo di quel veleno. Volli farne la prova in tre cunigli, e in due Piccioni, che tenni lungamente digiuni. Muorirono tutti in meno di 35 minuti, con soli tre grani di veleno. Ripetei l'esperimento in altri 5 animali, come sopra, ma a stomaco pieno: non ne mori, che un solo.

Ne deduco, come una verità di fatto, che il veleno Americano preso per bocca è veleno, ma che se ne richiede una quantità sensibile per uccidere un animale anche piccolo. I fatti riportati sopra sul veleno Americano, che in piccola dose è micidiale, mi farebbono credere, che il veleno della vipera, che è innocente preso per bocca in piccola quantità, fosse poi mortale preso in mag-

gior quantità. Quel sentimento di torpore che eccita sulla lingua, e che dura si lungamente, par che basti per non lo credere affatto inattivo, e che preso in gran quantità potrebbe benissimo dar la morte. Mi reservo di far questa esperienza in qualche altra occasione, e allora farò uso del veleno di 18, in 20 vipere, che darò a mangiare ad un piccolo animale a stomaco vuoto, e ardisco presagire d'avanza che probabilmente morra, perché se in piccolissima dose toglie il moto, e il senso alla lingua, cioè i principi della vita in quell' organo; una piu gran quantità dovrà torgli anche agli organi; i piu essenziali alla vita medesima. Se si riflette, che preso il veleno per bocca deve estendersi sopra una superficie grandissima, sempre umida, e mescolarsi coi cibi del ventricolo, che i vasi inalanti sono minimi, non parrà piu strano, che non nuoca quando è preso in piccola quantità, come si vede appunto del veleno Americano.

Cominciai le mie esperienze sopra l'attività di questo veleno col ferire con una lancetta imbrattata di veleno sciolto nell' acqua, diverse parti degli animali. Ferii con essa un piccolo porchetto d'India per tre volte nella coscia a diverse distanze. La lancetta era piena di veleno, ma l'animale non soffri nulla. A trè altri piccoli porchetti, e ad un cuniglio feci le medesime prove, e non ne mori nessuno, nè mostrorono di soffrir nulla. In tutti questi casi esciva il sangue sensibilmente dalle ferite, onde sospettai,

pettai, che il veleno non poteſſe comunicarſi, ma che foſſe ſpinto addietro, come avevo obſervato del veleno della vipera, che ſpeſſo non nuoce per queſta ragione.

Il mio ſoſpetto venne ben preſto confermato dalle ulteriori eſperienze. Inzuppai un filo ſemplice di veleno, e con eſſo trapaſſai la pelle d'un porchetto d'India in vicinanza di una poppa, non ſoffrì malattia alcuna. Inzuppai un nuovo filo a trè doppi, e lo laſciai prima aſciugare un poco, temendo che il veleno reſtaſſe addietro ſulla pelle nel paſſare il filo per eſſa. Lo feci paſſare attraverſo la pelle d'una coſcia d'un piccolo cuniglio in vicinanza della pancia. Il cuniglio dopo 6 minuti cominciò a tremare, e a moſtrarſi debole. Dopo un altro minuto cadde ſenza poterſi più muovere. Di quando in quando dava delle piccole convulſioni. Morì dopo altri ſei minuti.

Queſta medeſima eſperienza a fili doppi inzuppati fu da me ripetuta in altri due cunigli, e in trè porchetti d'India, e tutti morirono dentro 30 minuti, e caddero ſenza forza, e convulſi dopo 6, in 7 minuti.

Ero curioſo di vedere ſe il veleno Americano poteva comunicarſi agli animali, e uccidere, applicato ſemplicemente alla pelle grattata, o appena ferita colla punta d'una lancetta. Avevo oſſervato a Parigi, che il veleno della vipera comunica benſì una malattia locale in quei caſi, e che altera, e corrompe la pelle, ma che non arriva

ad

ad uccidere. Il veleno Americano per l'opposto non produce mai alcuna malattia locale, come avevo osservato, nel far l'esperienze riportate di sopra, e lascia le parti ferite nello stato naturale; il che forma una differenza essenziale fra il veleno della vipera, e il veleno Americano.

Levai il pelo colle cesoje ad un piccolo porchetto d'India sopra la pelle d'una coscia, e lo graffiai leggiermente con una Lima. Non esciva sangue visibilmente, ma si vedevano delle macchiette rosse, e la pelle inumidita. Bagnai la pelle con una gocciola di veleno sciolto nell' acqua. Dopo 10 minuti dette dei segni di aver le convulsioni, poco dopo cadde, senza piu muoversi, e solo di quando in quando aveva delle convulsioni piu e meno grandi. Mori dopo 20 minuti, La pelle dove si era applicato il veleno non era punto alterata. Questa esperienza ebbe il medesimo esito sopra due altri porchetti d'India, e sopra un piccolo cuniglio, che morirono tutti tre in meno di 27 minuti coi segni piu manifesti di convulsioni. Volli vedere se gli animali piu grandi potevano reggere a questo veleno applicato solo alla pelle graffiata. Colla punta d'una lancetta ferii leggiermente in molti luoghi la pelle prima scoperta dei peli, di un gran cuniglio, e la bagnaì con piu gocciole di veleno. Dopo 15 minuti diventò meno vivace di prima,

e crollava

e crollava il capo di quando in quando, quasi che non lo potesse sostenere, che con difficoltà, ma in meno di altro 20 minuti ritornò cosi vivace, che prima. Ripetei l'esperienza sopra un altro cuniglio, ma piu piccolo. Dopo 10 minuti dava de crolli col capo, appena poteva caminare, e reggersi sulle zampe, ma dopo altri 20 minuti ritornò cosi vivace, che prima.

Rasai circa un pollice di pelle con un rasojo ad un coniglio assai grande. Escì un poco di sangue, benchè non si vedessero i tagli. Messi sopra la pelle circa 3 goccie di veleno. Dopo 6 minuti il cuniglio dette segni di star male, e di esser molto debole. Un minuto dopo cadde, come se fosse stato morto: appena respirava sensibilmente; di quando in quando aveva delle convulsioni. In meno di 46 minuti si riebbe a segno che caminava molto bene, e cominciò poco dopo a mangiare, senza aver piu segno di male veruno.

Graffiai la pelle d'una coscia ad una gallina, e vi applicai il veleno. Non soffrì nulla, benchè ripetessi l'esperienza due altre volte in altre parti della pelle.

Scarificai leggiermente la pelle d'una coscia ad un Piccione, e vi applicai il veleno sciolto nell' acqua. Dopo 25 minuti era si debole, che non si reggeva piu in piede, ed aveva delle convulsioni per intervalli. Cadde poco dopo, come se fosse stato morto, e stette in questo stato di morte

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apparente per piu di trè ore. Poco a poco però cominciò a rimetterfi, a fegno, che dopo mezza ora pareva che non aveffe avuto mai nulla.

Queft' efperienza fopra i Piccioni fu replicata cinque altre volte. Tre morirono in meno di 20 minuti, e gli altri due caddero in convulfione, ma fi riebbero alla fine.

Da altre efperienze fatte dopo, tanto fopra i volatili, che fopra i quadrupedi ho potuto concludere, che il veleno Americano applicato fopra la pelle appena graffiata un poco può dar la morte, benchè non fempre, nè in tutte le circoftanze. Gli animali piu grandi refiftono piu facilmente all' azione di quel veleno; quando poi gli animali anche più deboli non muojono, fi trovano dopo poco tempo così bene che prima.

Defideravo di fapere, che quantità di veleno fi richiedava, per uccidere un' animale. Una fimile ricerca io avevo fatto in Francia ful veleno della vipera, dove avevo determinato la quantità di quel veleno, che fi richiedeva per ammazzare i diverfi animali. Potevo ben prefumere, che pochiffimo veleno Americano baftaffe per uccidere un piccolo animale, giacchè una, o due gocciole applicate alla pelle graffiata aveva potuto togliere la vita a piu d'uno. Ma volevo qualche cofa di precifo.

Toccai un fiocchetto appena visibile di Cotone, con circa $\frac{1}{50}^{mo}$ d'una gocciola di veleno sciolto nell' acqua, il quale poteva essere appena la $\frac{1}{50}^{ma}$ parte di tutta la gocciola. Introdussi in un muscolo della coscia d'un Piccione il fiocchetto di Cotone, e l'animale non mostrò di soffrir nulla.

Due ore dopo messi in un' altro muscolo un atomo di veleno secco, che appena vedevo cogli occhi, nè il Piccione soffrì per questo. Replicai l'esperienza del veleno secco in tre altri Piccioni, in un de' quali il pezzetto di veleno secco era molto sensibile, ma nessuno mori, o mostrò di soffrire. Feci la stessa esperienza sopra tre porchetti d'India, e sopra due piccoli cunigli col medesimo successo, e nessuno mostrò neanco di soffrire. Il veleno per altro non era stato sciolto dagli umori della parte ferita, e trovai i pezzetti di esso veleno affato intieri.

Applicai sopra un muscolo d'un altro Piccione un fiocchetto di cotone molto maggiore che quei di sopra, e vi applicai circa 8 volte piu di veleno. Il Piccione cadde dopo 6 minuti, e poco dopo mori. Applicai ai muscoli di due porchetti d'India dei fiochetti di cotone imbevuti di veleno appresso a poco come sopra. L'uno mori dopo 12 minuti, l'altro cadde come morto dopo 6, ma si riebbe poco dopo.

 Da

Da queste esperienze ne concludo, che si richieda circa $\frac{1}{100}$ di grano di veleno per uccidere un piccolo animale, e che è necessario, che il veleno si sciolga per dar la morte, o per cagionare qualche disordine nell' economia animale.

Varie esperienze ho fatto per determinare se il veleno Americano applicato alle creste ferite delle galline, o alle orecchie graffiate dei quadrupedi; era micidiale o pericoloso. Il veleno della vipera non è ordinariamente micidiale in quelle parti, e la malattia non si manifesta nella cresta avvelenata, ma bensì nelle barbe, che gonfiano orribilmente, e a segno di fare spesso morir l'animale.

Ho adunque ferite piu volte la cresta delle Galline, vi ho applicato il veleno Americano, ve l'ho insinuato due volte col cotone bene inzuppato di esso veleno, senza aver mai potuto produrre alcuna malattia. Ma riesci l'esperimento meglio nelle orecchie. Dopo di aver fatti piu tentativi tutti infruttuosi per communicare il veleno graffiando, o feriendo le orecchie di piu cunigli, che non mostrarono di soffrir nulla; alla fine mi riesci di farne morir due in meno di 30 minuti, dopo di aver loro applicato una gran quantità di veleno nelle parti piu carnose delle orecchie, che avevo ferite in moltissimi luoghi colla punta della lancetta.

Le esperienze delle orecchie mi avevano fatto vedere, che dove vi sono pochi vasi sanguigni, o non si comunica

la

la malattia, o non è mortale. In queſto il veleno Americano ha molta analogia con quello della vipera. Come il veleno della vipera è innocente affatto meſſo ſopra i tendini e ligamenti, ſpecialmente ſe privati di vaſi roſſi; così il veleno Americano meſſo ſopra quelle parti è egualmente innocente. E ſuperfluo che io porti il dettaglio di queſte eſperienze, che non potrebbe eſſere che troppo lungo, e non affatto neceſſario, come ſi vedrà dopo.

Ero deſideroſo di ſapere, ſe il veleno Americano inſinuato ne muſcoli era piu micidiale, che applicato alla pelle, anche traforata da banda a banda. Un groſſo porco d'India, che aveva ſofferto due giorni avanti per due volte l'operazione nella pelle tagliata, ſenza ſoffrir alcuna malattià, ed una terza volta con piccoli ſegni di malattià, morì in meno di 12 minuti dopo di avergli meſſo del veleno ſulle fibre tagliate d'un muſcolo della coſcia. Dopo tre minuti cadde quaſi ſenza ſegni di vita, e con perdita totale di moto. Queſta eſperienza l'ho fatta dieci altre volte, e tutti gli animali ſono morti, tanto i porchi d'India, che i Piccioni, e cunigli di mediocre grandezza; talchè non poſſo dubitar che le ferite avvelenate nei muſcoli non ſieno piu micidiali di quelle fatte nella cute, nelle orecchie, e nelle creſte delle Galline. Il metodo piu certo però di rieſcire ſi è di inzuppare bene nel veleno un pezzetto di legno ſpugnioſo, e tagliente, è di inſinuarlo coſi

A a 2 quaſi

quasi seccato nella sostanza del muscolo scoperto. Mà questo metodo non riescì per altro per tre volte, che ne feci uso sopra le creste delle Galline. Non potei vedere nessun segno di malattià, benchè il legno fosse bene inzuppato, e lo lasciassi per piu ore nelle creste traforate.

In questa occasione feci uso delle freccie: molte ne adoprai traforando la pelle degli animali, e molte altre traforando i muscoli. Non tutti gli animali, specialmente fra i grossi cunigli, benche feriti con esse nella pelle morirono, sebben peraltro ne morisse la piu gran parte; ma nessuno ne guari fra quelli a cui avevo traforato con esse i muscoli. In generale ho trovato, che le freccie sono piu pericolose, e piu micidiali, che il veleno sciolto nell' acqua, quando è semplicemente applicato alle parti ferite. Ho trovato, che è piu attivo il veleno delle freccie se si bagniano prima nell' acqua calda, e che allora operano piu sicuramente, e piu prontamente, e cresce ancor di piu la loro attività, se si inzuppano prima nel veleno bollito nell' acqua a consistenza di giulebbe. Vari animali anche grandi come i cunigli sono caduti senza potersi piu muovere in meno di due minuti; qualcheduno de piccoli ha mostrato di soffrire in meno di un minuto.

Insinuai una freccia ben inzuppata prima nel veleno bollito nella cresta d'una gallina, e ve la lasciai un giorno

4 intiero,

intiero, ſenza che l'animale aveſſe dato ſegno di ſoffrir nulla. Il giorno dopo le traforai la creſta, e le barbe con due freccie preparate, come ſopra, e ve le laſciai per 10 ore. La gallina non ſoffri nulla ne anco in queſta ſeconda eſperienza. Allora gli inſinuai una freccia attraverſo un muſcolo delle coſcie, e mori in 42 minuti.

Fra le richerche, che mi ero propoſto nell' eſame di queſto veleno, ve ne era una ſopra le alterazioni, che poteva ſoffrire unendolo agli acidi, e agli alcalini, come avevo fatto del veleno della vipera. Avevo trovato, che nè gli acidi minerali piu potenti, nè gli alcalini piu attivi toglievano le qualità mal facienti al veleno della vipera. Per queſto fine ſciolſi nei tre acidi minerali il veleno, e ne ſciolſi ancora nell' aceto diſtillato, e nel rum, e dopo qualche ora feci le ſeguenti eſperienze.

Feci dei piccoli tagli ſopra la pelle d'un piccolo porco d'India, e la coprii piu volte di veleno ſciolto nell' acido nitroſo. L'animale non parve di ſoffrir nulla, che l'incomodo meccanico della ferita, e dell' acido. Dopo un ora era tanto vivace, che prima. Dopo due ore ripeteì l'eſperienza in un altra parte della pella preparata come ſopra, che coprii di veleno ſciolto nel Rum, e l'animale mori in meno di 4 minuti.

Ferii leggiermente la pelle d'un piccolo cuniglio, e vi applicai ſopra piu goccie di veleno ſciolto nell' olio di

vetriolo.

vetriolo. Non mostrò di soffrir nulla, ed era così vivace che prima. Dopo 4 ore preparai un altra parte della pelle come sopra, e vi applicai poche goccie di velena sciolto nell' aceto distillato. Morì in 6 secondi, e cascò in meno di quattro.

Preparai la pelle al solito d'un altro piccolo cuniglio, e la copersi con veleno sciolto nell' acido marino. L'animale non parve che soffrisse nulla, dopo 6 ore applicai il veleno sciolto nel Rum ad un altra parte della pelle. Dopo 45 minuti cadde con convulsioni, ma si ristabilì in meno d'un ora.

Da queste prime esperienze parrebbe, che gli acidi minerali rendessero innocente questo veleno, e che per lo contrario l'aceto, e il Rum non vi facessero alcuna alterazione. Continuai le mie esperienze sopra il veleno sciolto nell' aceto, e nel Rum, e i resultati furono un poco vari. Di sei animali trattati col veleno sciolto nell' aceto, due soli morirono, due ebbero tutti i segni della malattia di veleno, e due altri non ebber nulla. Di sei altri trattati col veleno sciolto nel Rum ne morirono cinque, e il sesto ebbe la malattia del veleno; onde par dimostrato, che il veleno sciolto in que' due fluidi conserva le sue qualita micidiali.

Per l'opposto ho ripetute le esperienze del veleno sciolto negli acidi minerali sopra sei animali, e nessuno è

morto,

morto, o ha mostrato di aver nessun principio di malattia di veleno. Mi venne il sospetto, che forse il veleno non uccidesse non perchè avesse perdute le sue qualità micidiali, ma piuttosto perchè non poteva insinuarsi nelle parti ferite a motivo della troppo grande azione degli acidi minerali sopra la pelle, e sopra i vasi, che raggrinzano e brucciano in qualche maniera. Per rischiarirmi di questo dubbio feci evaporare al fuoco il veleno sciolto negli acidi minerali, e fatto secco lo applicai piu volte a piu animali in diverse parti della loro pelle. Ma nessuno dette segno di soffrire.

Pare adunque, che gli acidi minerali tolgano le qualità nocive al veleno Americano: dissi semplicemente, che pare, perchè si potrebbe forse sospettare, che rimanendo un poco di acido unito al veleno dopo evaporato, si producesse la solita alterazione ne vasi della pelle. Avrei dovuto ripeter qualche altra esperienza dopo di averlo piu volte lavato in acqua, e reso insipido, ma in quel tempo mi sono mancati gli animali per verificar questo nuovo sospetto, e dopo non ho piu avuto tempo di ritornare sopra questa materia.

Intorno ai sali alcalini posso dire, che non sono accorto, che avessero alterato quel veleno per nessun conto, e reso meno micidiale di prima; e bensì vero, che queste esperienze non le ho tanto ripetute, ne si variate, come si dovrebbe,

dovrebbe, e come avrei ancora fatto, so non avessi trovata gran difficoltà in procurar gli animali, e non avessi avuto in vista della esperienze piu importanti assai.

Era naturale che io sospetassi, che siccome gli acidi impediscono l'azione del veleno contro gli animali, potessero ancora essere un rimedio contro quel veleno.

Preparai al solito la pelle d'un porchetto d'India, e la copersi tutta con veleno, e circa 40 secondi dopo la lavai con acido nitroso, e poi con acqua pura. L'animale non soffri nulla. Dopo due ore lo avvelenai in un muscolo, e vi applicai subito l'acido nitroso, ma cadde nel momento convulso e senza forza, ed era morto dopo 2 minuti.

Ripetei quest' esperienza nei muscoli d'un altro porchetto d'India, ed appena avvelenato, gli lavai con acido nitroso, e un poco d'acqua. Dopo due minuti cadde convulso, ed era morto dopo 4.

Avuelenai, come sopra i muscoli di 4 piccioni, e nel momento dopo gli lavai con acido nitroso. Morirono un minuto dopo. Dubitando che fosse affetto dell' acido nitroso piuttoso che del veleno, feci uso di acido nitroso molto indebolito sopra 4 altri piccioni, ma morirono tutti 4, benche assai piu tardi. Volli vedere, se la semplice applicazione dell' acido nitroso ai muscoli poteva uccidere i piccioni, e i piccoli porchetti d'India, lo sperimentai sopra due

due piccioni, e ſopra due porchi d'India. I piccioni morirono tutti due poco dopo, ma non già i porchetti d'India, benchè uno moſtraſſe di aver molto ſofferto.

Mi pare adunque, che gli acidi ſieno un rimedio inutile, o pericoloſo applicato ai muſcoli avvelenati dell' animale.

Nulla dirò di qualche altro rimedio, che ho adoprato, perchè ho trovato coll' eſperienza, che tutto è inutile, o ſi applichino preſto, o tardi, o eſternamente, o internamente. Quando il veleno è inſinuato profondamente, quando ſi è già introdotto negli umori, qualunque rimedio è gia tardo, ed inutile.

Mi reſtava benſi da fare una ricerca aſſai piccante, e che avrebbe potuto ancora in qualche caſo eſſere utile. Le mie eſperienze ſopra il veleno della vipera mi hanno data occaſione di far la medeſima ricerca ſopra il veleno Americano. Avevo determinato con eſſe il tempo che impiega il veleno della vipera per diffonderſi nel corpo dell' animale; e quando poteva eſſer utile di recider la parte avvelenata, o di far delle legature per impedire, che il veleno ſi comunichi col mezzo del ſangue all' animale.

Introduſſi ne' muſcoli d'una gamba d'un piccione una freccia Americana bagniata prima nell' acqua calda. Dopo quattro minuti feci una legatura mediocremente forte al di ſopra della parte ferita, ſubito ſopra del

e vi lasciai la freccia. Dopo 26 ore l'animale non parve che avesse altra malattia che della semplice legatura. Allora levai la freccia, e sciolsi la legatura. La parte era un poco gonfia, e livida; ma l'animale non morì per questo, benchè non potesse far uso della gamba, che dopo molti giorni, e con qualche stento.

Passai con una nuova freccia i muscoli d'un altro piccione, come sopra, e dopo sei minuti vi feci la legatura, e vi lasciai la freccia. Dopo altri quattro minuti il piccione non aveva piu forza per sostenersi, o tener ritto il capo. Poco dopo cadde come se fosse morto, e morì infatti dopo altri sei minuti.

Ripetei la medesima esperienza sopra d'un altro piccione, e lasciai nei muscoli la freccia. Dopo 8 minuti legai la gamba, dopo 3 altri minuti cominciò a dar segni di star male, ma poco dopo si riebbe. Dopo 26 ore viveva ancora, benchè i muscoli fossero lividi. Levai la legatura, e dopo due ore morì.

Sottoposi un piccione alle istesse esperienze, e feci l'allaciatura dopo 5 minuti, lasciando la freccia nè muscoli. Morì dopo due ore.

Feci le stesse esperienze sopra altri quattro piccioni, ai quali feci la legatura dopo due minuti. Nessuno morì di essi; dopo 10 ore levai le legature, e ne morirono tre; il quarto guarì perfettamente.

Ripetei

Ripetei ſopra altri quattro piccioni le medeſime eſperienze nelle ſteſſe circonſtanze, e levai le legature dopo 30 ore. Ne morì un ſolo, e morì dopo due giorni, certamente per effetto della legatura troppo forte, che aveva prodotta la gangrena né muſcoli. Queſte ſteſſe eſperienze ſono ſtate da me ripetute ſopra piccioni molto piu giovani, ai quali ſi può tagliare ſenza che muojono, la gamba ſotto il femore. Non ne morì neſſuno di quelle ai quali avevo tagliata la gamba dopo due minuti, e due di dieci morirono, ai quali avevo tagliata la gamba dopo 3 minuti.

Con queſto metodo muojono meno piccioni, che colle legature, quaudo ſi fa uſo di eſſe nello ſteſſo tempo. La ragione è, che il taglio non produce morte, né alcuno ſconcerto notabile in queſti animali, laddove la legatura fa ſpeſſo ingangrenire le parte ferite dalle freccie, e il piccione muore ſpeſſo della gangrena. Le medeſime eſperienze ho fatte ſopra i piccioli porchi d'India, e i piu piccoli cunigli, quando tagliando le gambe, quando facendo le legature. I riſultati ſono ſtati in parte analoghi a quelli oſſervati nei Piccioni, benchè un poco meno coſtanti, e piu incerti.

In generale ho veduto, che ſi richiede un dato tempo, perché il veleno Americano ſi comunichi all' animale, che queſto tempo è molto piu grande che quello che ſi richiede, perchè ſi comunichi il veleno della vipera, e

che gli effetti sopra gli animali sono piu vaghi, e piu variati nel veleno Americano, e che finalmente si può guarire dall' un veleno, e dall' altro col recider le parti, quando si posson recidere senza rischio della morte, e si faccia il taglio in tempo.

Nelle mie esperienze fatte sopra il veleno della vipera ho trovato che non è poi veleno per tutti gli animali, e che vi sono degli animali a sangue freddo, per i quali è affatto innocente. Ero curioso di vedere se seguiva l'istesso del veleno Americano. Tutti i scrittori del veleno Americano ci dicono, che è veleno per tutti gli animali, ma il credere una cosa è ben lontano dal provarla. Ci vogliono delle esperienze, ce ne voglion moltissime, e non si vede che ne abbiano fatte abbastanza per cavare una illazione si generale.

Cominciai dall' insinuarlo nei muscoli delle rane, le quali morirono in poco tempo. Passai alle anguille, nelle quali insinuavo delle freccie verso la coda, e morirono tutte benchè molto tardi.

Avevo trovato il veleno della vipera affatto innocente per la vipera medesima, e per quei serpenti, che in Toscana si chiamano binchi, e dai francesi sono chiamati couleuvres. Di questi ultime non ne potei avere che due soli, onde non feci che poche esperienze, benche io le creda decisive affatto. Ne ferii uno con una freccia bene im-

brattata di veleno a confiſtenza di ſciroppo, verſo la coda, e vi laſciai la freccia nei muſcoli. Nel luogo per dove inſinuai la freccia avevo prima fatto una inciſione, perchè poteſſe entrare facilmente nei muſcoli anche il veleno ſciolto, che era ſopra la freccia. Nel luogo poi della ferita v'inſinuai del nuovo veleno facendo de' piccoli tagli nei muſcoli. Il ſerpente non moſtrò di ſoffrir nulla, e dopo piu ore era coſi bene che prima. Lo ferrai dentro di una caſſa, la quale avendo aperto dopo altre ſei ore trovai, che il ſerpe era fuggito, nè potei piu ritrovarlo dopo. In un altro, un poco minore, ripetei l'eſperienza piu volte a diverſi intervalli. L'ultima volta inſinuai due freccie avvelenate nei muſcoli della coda, e ve le laſciai per 24 ore. Intorno alle ferite vi applicai piu volte il veleno a conſiſtenza de ſciroppo, e ve lo introduſſi a gran doſi con uno ſtecco. L'animale non morì, e non parve che ſoffriſſe ſenſibilmente.

Queſta ſteſſa eſperienza ho potuto però farla piu volte ſopra le vipere, neſſuna è morta per il veleno, benchè qualcuna foſſe ferita nei muſcoli verſo la coda di piu freccie, e bene ſpalmata di veleno a conſiſtenza di ſciroppo. Laſciavo le freccie per 20, e 30 ore nei muſcoli, nè per queſto ne morì mai alcuna. E benſi vero, che alcune poco dopo avvelenate parevano meno vivaci di prima, e pareva, che la parte ferita, o la metà poſteriore

del

del corpo avesse perduto sensibilmente del suo moto naturale, e questo torpore durò in alcune per molte ore, altre poi erano sempre tanto vivaci, che prima.

Non dubito dopo tutto questo di asserire, che il veleno Americano è affatto innocente per questi animali a sangue freddo, come lo è il veleno della vipera; nella qual cosa questi due veleni hanno una analogia molto grande: benchè l'uno non sia che una gomma animale, come ho dimostrato altrove, e l'altro un semplice succo vegetabile.

Mi rimaneva di essaminare l'azione di questo veleno sopra gli animali viventi; o sia, quali sieno le parti alterate dal veleno Americano nell' animale, per cui ne segue la morte.

Tutto concorreva a far credere, che eccitasse una di quelle malattie, che dai medici moderni son chiamate nervose. I sintomi della malattià soni i piu precisi, e i piu decisi per quel genere di malattie. Convulsioni, debolezze, perdita totale di forze, e di moto, sentimento diminuito, o quasi tolto affatto; sono i sintomi piu comuni di quel veleno negli animali. Spesso si osserva, che l'animale da vivace, che era un momento prima, si trova un momento dopo senza moto, e senza senso, e prossimo alla morte. Ho osservato in generale un sintoma che pare una vera dimostrazione, che la mallatià prodotta da questo veleno sia parimente nervosa. Se l'animale non muore

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in pochi minuti, si trova tanto bene che prima, e non pare che habbia sofferto nulla, benchè sia rimasto in uno stato di letargo tal volta per piu ore, senza segni certi, e manifesti di vita. Questo è appunto il caso di quelle malattie che si chiamano nervose: vengon spesso ad un tratto, risvegliano quando de' moti, e quando privano affatto di forze, ma appena cominciano a dissiparsi gli effetti della malattià, che la persona si trova benissimo, e appena si accorge d'aver sofferto qualche male. Ma tutti questi segni non potranno piu impormi dopo le mie esperienze sopra il veleno della vipera: anche la malattià prodotta da quel veleno ha dei sintomi delle malattie nervose, e pare, che i nervi sieno affetti principalmente, benchè l'esperienza abbia deciso il contrario. Bisogniava dunque anche quì ricorrere all' esperienza, e non si lasciar sedurre da teorie male imaginate, e da ragioni apparenti.

Per procedere con metodo in una questione cosi importante, ho creduto bene di cominciare dall' esaminare se il veleno Americano produce qualche alterazione sensibile sopra il sangue degli animali cavato dei vasi, e messcolato con esso.

Ho reciso il capo ad un piccione, ed ho ricevuto in due bicchieri tepidi, e conici, il sangue ancor caldo d'un piccione. In ciascuno dei bicchieri ne feci cadere circa 80 gocciole. In uno de bicchieri messi quattro gocciole di

acqua, nell' altro quattro gocciole di veleno sciolto parimente nell' acqua. La quantità del veleno delle quattro gocciole, appena arrivava ad un grano in peso, quando era secco. Nel medesimo istante girai i due bicchieri per pochi secondi egualmente, e in modo che le materie si mescolassero. Dopo 2 minuti il sangue mescolato coll' acqua, si era coagolato. L'altro sangue unito al veleno Americano non si coagolò mai, ma era in vece piu oscuro, e piu nero dell' altro, che era rosseggiante al solito. Dopo 3 ore era ancor fluido come prima, quando nell' altro bicchiere si vedeva, che il siero si era già separati dalla parte rossa.

Esaminai col microscopio allora, e dopo, il sangue de due bicchieri, e trovai che nell' uno, e nell' altro i globetti rossi conservavano la loro prima figura, e che non differivano punto fra di loro.

Questo esperimento ripetuto piu volte ha sempre avuto il medesimo successo, talchè par cosa evidente, che il veleno Americano non altera sensibilmente i globetti rossi del sangue nelle circostanze accennate. Non lascia però di esser degno d'attenzione, che questo veleno è si lontano dal coagulare il sangue, che anzi impedisce assolutamente che egli si coagoli, come segue quando il sangue è cavato dai vasi; nè si può dire che egli attenui, o disciolga il sangue, perchè nulla di questo si osserva, quando si esamina

esamina al microscopio. La parte rossa è figurata come nello stato naturale, e nulla si osserva di piu sottile, e di piu fluido in quell' umore.

Una cosa affatto simile abbiamo osservato ancora succedere col veleno della vipera, tal chè gli effetti, o alterazioni fatte da queste due veleni sul sangue cavato dai vasi pajono affatto simili; l'uno e l'altro veleno impediscono, che il sangue si coagoli, e nè l'uno ne' l'altro scioglie, o altera i globetti del sangue, e la sola differenza fra essi consiste, che il veleno della vipera tinge assai piu in nero il sangue, che il veleno Americano.

Il veleno della vipera non altera i globetti, nè anco quando si comunica all' animale vivente, e che ne segue la morte dell' animale medesimo. L'istesso ho osservato nell' sangue di quelli animali, che sono morti dal veleno Americano, talchè i due veleni convengono mirabilmente in tutti questi casi. Ma si è veduto, che il veleno della vipera produce una alterazione sensibile sopra la massa del sangue in generale negli animali morsicati; la stessa attenzione ho creduto di dover portare nell' esame del sangue degli animali morti per il veleno Americano.

In generale mi è parso, che i muscoli degli animali morti dal veleno Americano fossero piu pallidi di prima. I vasi venosi verso il cuore mi sono parsi piu turgidi del solito. Il sangue un poco piu oscuro dell' ordinario, ma

non moltiſſimo, e non coagulato. I viſceri del baſſo ventre non ſenſibilmente alterati, il cuore, e le orecchiette in ſtato naturale, il cuore però par che abbia qualche volta i ſuoi vaſi eſterni piu viſibili, e quaſi injettati.

Ma ho oſſervato però una grande alterazione in un viſcere dei piu eſſenziali alla vita. Il polmone mi è parſo ſempre molto alterato. L'ho trovato generalmente macchiato piu, e meno, e ſpeſſo di macchie aſſai larghe, e livide. In alcuni ſi ſarebbe creduto, che foſſe tutto putrefatto. Queſta alterazione in un viſcere ſi eſſenziale alla vita merita la piu grande attenzione, e mi è parſo, che ſia tanto piu grande, quanto più l'animale è viſſuto, dopo di eſſere ſtato avvelenato. Ho oſſervato il polmone di alcuni animali eſſer qua e là traſparente, ſpecialmente verſo i lembi. Si vedeva beniſſimo l'aria polmonare attraverſo della membrana eſterna. L'ho eſaminato col microſcopio, ed ho oſſervato beniſſimo le piccoli veſcichette polmonare irrigate di vaſi per la piu gran parte privati di ſangue.

Per quanto foſſe grande queſta alterazione in un viſcere tanto importante non ſapevo affatto perſuadermi, che ſola poteſſe produrre una malattià coſi grande, e coſi momentanea, e che tutta l'azione del veleno foſſe ſolamente contro il ſangue, e contro il polmone. E vero, che

avevo

avevo l'esempio del veleno della vipera, che produce qualche cosa di simile, ma questo veleno produce un coagulo quasi generale del sangue medesimo, che non si osserva certamente nel veleno Americano.

In una ricerca così importante, e sì oscura nel tempo stesso, ho creduto di dover ricorrere all' esperienza medesima, e di esaminare gli effetti del veleno Americano introdotto immediatamente nel sangue. Mi son servito dei medessimi mezzi che ho adoperati per introdurre nel sangue della jugulare il veleno delle vipere. Un sifoncino di vetro ricurvo in punta faceva le veci d'una piccola siringa. Con questo sifoncino assorbivo il veleno Americano sciolto prima nell' acqua, e aperta la vena jugulare lo spingevo dentro di essa. Siccome il metodo di far questa sorta di esperienze è di già descritto nelle mia opera sul veleno della vipera, ho creduto di non dover qui darne a parte una descrizione. L'esperienza è talmente condotta, che il veleno entra per la jugulare nel sangue, senza toccare a nessuna parte tagliata del vaso, neanco della jugulare medesima.

Quattro gocciole di veleno sciolto nell' acqua io messi nella siringa di vetro per la prima esperienza. La quantità del veleno nelle quattro gocciole appena poteva montare ad un mezzo grano. Introdotto il becco ricurvo della siringa nella jugulare d'un grossissimo cuniglio,

 nell'

nell' atto di spingere lo stantuffo, m'accorsi che il veleno era ritornato indietro, a motivo che lo stantuffo non s'accostava bene alle pareti della siringa, onde dissi alle persone, che erano presenti, che l'esperienza era mancata. Ma restai sorpreso quando sentii dirmi, che l'animale era già morto. Io non credo, che vi corressero dieci secondi dal momento in cui viddi il veleno ritornare indietro, al sentir dire che l'animale era già morto, e lo era in fatti. Jo non posso dire, che quantità di veleno sia stata introdotta nel sangue, se l'animale è morte bisogna pure che ne sia stata introdotta una quantità sufficiente; senza di questo io avrei giudicato dalla quantità del veleno ritornato addietro, che neppure un atomo ne fosse entrato nella jugulare.

L'animale era talmente morto, che non appariva alcun segno o moto di respirazione, e tutto il corpo era cosi cascante e rilasciato in tutte le parti, che non si trova negli animali neanco morti da lungo tempo. La morte di questo animale è stata cosi vicina all' introduzione del veleno che non è parso che vi corresse nessun tempo sensibile: mi è parsa molto piu pronta, che ne' casi del veleno della vipera introdotto nel sangue nell stesse circostanze.

Rimessa la mia siringa in migliore stato v'introdussi due gocciole sole di acqua a cui avevo prima unito $\frac{1}{4}$ di gocciola del veleno di sopra sciolto nell' acqua. Appena

incominciai a ſpingere il veleno per la jugulare, che veddi il cuniglio cader morto, come ſe foſſe ſtato toccato dal fulmine. Jo non credo che foſſe introdotto nel ſangue mezza gocciola del liquor della ſciringa, quando l'animale cadde ſenza moto, e ſenza vita.

In generale mi par di poter dire da altre eſperienze fatte poi, che queſto veleno introdotto immediatamente nel ſangue per la jugulare uccide piu preſto, e in minor quantità del veleno della vipera. La morte ſegue coſi da vicino l'introduzione del veleno nel ſangue, che previene ordinariamente le convulſioni dell' animale. Se ſi prende una minor quantità di quel veleno allora ſi oſſervano le ſolite convulſioni, e battimenti, e la morte non ſegue coſi ſubito.

E vero che il ſangue non è coagulato, ne sì alterato nel colore, come quando s'introduce nella jugulare il veleno della vipera, ma non per queſto la morte ſegue piu tardi, e non è men certo, che il veleno Americano introdotto nel ſangue immediatamente, come il veleno della vipera, uccide nella ſteſſa maniera gli animali.

Queſta è une verità di eſperienza, a cui nulla vi é da opporre, comunque poi poſſa eſſere oſcura, o poco s'intenda la cauſa della morte nei caſi di ſopra. Il veleno Americano introdotto nel ſangue uccide l'animale nell' iſtante, onde pare ancora indubitato, che quando ſi applica

plica esteriormente ad una parte ferita d' un animale vivente, possa e debba ancora portare per mezzo del sangue de gravi sconcerti all' economia animale, e la morte medesima. La morte dell' animale, che seque nel momento, che si introduce quel veleno per le jugulari nel sangue, pare una dimostrazione, senza replica, che in quei casi tutta l'azione del veleno è contro il sangue medesimo, e che il sistema nervoso non è punto affetto, o alterato. Ma tutto questo non è ancora una prova, che i nervi non possano essere piu e meno affetti da quel veleno, quando la morte segue molto piu tardi, e quando si applica esternamente sulle parti ferite. In questi casi vi sono principalmente le convulsioni, e tutti i segni d'una malattià nervosa. Può adunque benissimo il nervo essere affetto dal veleno, ed esser la principal cagione della morte dell' animale.

Bisognavo per altro anche qui ricorrere all' esperienza diretta, come si è fatto del veleno della vipera, e vedere quali sconcerti, e malattie produce il veleno Americano applicato immediatamente sopra i nervi, senza toccare ai vasi.

Le mie esperienze sono state fatte sopra i nervi sciatici dei piu grossi cunigli, ed ho preparati quei nervi nella medesima maniera che ho fatto a Parigi operando sul veleno della vipera, e per questo io non darò qui alcun dettaglio

dettaglio risguardante il metodo di preparar questi nervi. Accennerò bensì un picciol numero di esperienze principali fatte su i nervi, perchè si vegga la varietà, che ho incontrato principalmente nei primi tentativi, i quali avrebbono potuto ingannarmi, se non mi fossi ostinato a moltiplicar le mie esperienze, e a variarle a proporzione, che trovavo dei risultati poco conformi. A questa costanza, o ostinazione, che si voglia chiamare, io devo principalmente le nuove verità, che credo di aver trovate sopra i due veleni della vipera, e del Ticunas.

Isolato il nervo sciatico ad un cuniglio vi passai per disotto un cencio fino a piu doppi, e posi sopra il nervo un fiochetto di fila ben imbrattato di veleno Americano a consistenza di sciroppo. Coprii il nervo con il medesimo cencio, perchè il veleno non scorresse nei muscoli scoperti dell' animale, e cucii al solito la pelle. Dopo 10 minuti il cuniglio cominciò ad aver delle convulsioni, a non piu reggersi in piede, a cader con tutti i segni dell' mallatià di veleno, e morì poco dopo.

Ripetei questa esperienza in un altro cuniglio, e procurai d' inviluppare con dei cenci, anche meglio, il nervo avvelenato, come sopra. Questo secondo cuniglio non mostrò di soffrir nulla per 10 ore di seguito, che l'osservai, ma dopo due altre ore lo trovai morto da poco prima, perchè era ancora caldo.

 Sospettai,

Sospettai, che il veleno applicato al nervo, essendo in qualche quantità notabile, potesse alla lunga penetrare, unitamente agli umori delle parti tagliate, attraverso i cenci, e portar la sua azione sopra i muscoli, e le parti adiacenti. Bisognava dunque, o scemare il veleno, o aumentare i cenci, e impedire qualunque diffusione di veleno attraverso di essi. Mi attenni a questo ultimo come piu sicuro.

Isolai il nervo sciatico al solito ad un cuniglio, e vi posi per disotto un cencio finissimo a moltissimi doppi. Collocai sopra il nervo il fiocco de' fili ben inzuppati nel veleno, e copersi ogni cosa coi lembi del cencio. Questo cuniglio visse 24 ore, e solo dette segnio di star male nell' ultima ora, ma senza, che potessi sospettare che morisse di malattià di veleno.

Preparai ad un nuovo cuniglio il nervo sciatico, come sopra, e lo coprii di veleno, e dei soliti cenci. Mori dopo 40 ore, senza segni di malattia di veleno.

Feci la medesima esperienza del nervo sciatico sopra altri tre cunigli, avendo tutta l'attenzione, che i nervi avvelenati fossero ben coperti dei cenci, e non vi fosse sospetto alcuno, che il veleno si potesse diffondere attraverso di essi. Uno morì dopo 3 giorni, e gli altri due vivevano ancora dopo 8 giorni.

Preparai

Preparai appunto come ſopra i nervi di due altre cunigli, ma ſenza veleno per fare una eſperienza di confronto. Un cuniglio morì dopo 36 ore, e l'altro viveva dopo 8 giorni.

Queſte eſperienze mi parevano ſufficienti, per giudicare ſe il veleno Americano applicato eſternamente ai nervi è capace di produrre qualche ſconcerto, o malattià nell' animale, ma mi reſtava da ſapere, ſe era egualmente inattivo quando ſi applicava ai nervi feriti, o ſia alla polpa medeſima dei nervi.

Preparai come ſopra il nervo ſciatico d'un cuniglio, e prima di applicarvi il veleno lo ferii piu volte con una lancetta da parte a parte. Appunto ſopra la parte ferita del nervo applicai il veleno. Il cuniglio viſſe cinque giorni, e morì ſenza ſegni di mallattià. Ripetei l'eſperienza ſopra un altro cuniglio colle medeſime circoſtanze, il quale viveva ancora dopo 8 giorni.

Variai un poco l'eſperimento ſopra i nervi di tre altri cunigli. In vece di far colla lancetta molti tagli aprii il nervo longitudinalmente per piu di cinque linee, e inſinuai per la feſſura i fili ben inzuppati di veleno, e coperſi bene ogni coſa. Uno mori dopo 60 ore, ſenza ſegni di mallattià di veleno, e gli altri due vivevano dopo 8 giorni.

Credetti di dover variare ancora queſta ſeconda ſorta di eſperienze, e di farne qualcheduna recidendo il nervo,

come avevo fatto esaminando il veleno della vipera. Recidevo il nervo sciatico il piu lontano, che potevo dal capo, perchè io potessi facilmente inviluppario coi cenci. La parte isolata del nervo nei piu grossi cunigli èra circa un pollice e mezzo. Collocato il nervo sopra i cenci, lo spalmavo bene di veleno nella parte recisa, e coprivo ogni cosa coi soliti cenci.

Questo esperimento lo feci sopra 6 cunigli, due morirono in 40 ore, due dopo 3 giorni, e due vivevano ancora nel quarto giorno.

Per fare una esperienza di confronto preparai come sopra i nervi di due cunigli, che recisi, ma non avvelenai. Uno morì dopo 36 ore, e l'altro viveva nel terzo giorno.

La costanza dei risultati di queste esperienze sopra i nervi mi ha fatto creder superfluo di ripeterne di piu, e ho creduto, che non lasciassero alcun dubbio in chi è avvezzo a sperimentare, e non è prevenuto per ipotesi mal provate. Quì si vede, che il veleno Americano non è veleno applicato comunque ai nervi, e che non produce alcun sensibile sconcerto sopra l'economia dell' animale vivente in quei casi. Questo è quello che depone l'esperienza immediata. Il supporre quello che non si vede, il credere quello, che è contraddetto dall' esperienza è sognare nelle cose fisiche, e correr dietro all' errore per

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la verità, e adottar l'immaginazione per il fatto. Il veleno Americano, ſimile in queſto al veleno della vipera non è veleno per i nervi, ed è un ſucco innocente comunque applicato ad eſſi, come lo è il veleno della vipera. Ma queſto veleno uccide nella piu piccola quantità, e uccide nel momento, ſe ſi introduce immediatamente nel ſangue per la jugulare, come fa il veleno della vipera; la ſua azione è adunque tutta contro il ſangue e non già punto contro i nervi, qualunque poi ſia il principio, o il meccaniſmo per cui ne ſegue la morte.

Gli effetti, e le alterazioni del veleno della vipera ſopra del ſangue ſono piu deciſi, e piu evidenti. Vi è un coagulo, che non ſi puo negare, e che non ſi oſſerva nel ſangue degli animali morti per il veleno Americano. Ma ſi vede però in queſti una grande alterazione nel polmone, e che quel viſcere è nel piu gran diſordine.

E vero che la morte ſuccede ſi ſubito injettando ſpecialmente il veleno Americano per i vaſi, che non ſi può ben comprendere come ſucceda la morte in ſi breve tempo: ſi direbbe, che appena il veleno è arrivato al cuore, che l'animale è già morto; nè ſi intende bene, come poſſano morire gli animali a ſangue freddo, come per eſempio le rane, che vivono ſi lungo tempo a circolazione arreſtata, benchè ſia vero, che muojono molto piu tardi per quei veleni degli altri animali a ſangue

 caldo.

caldo. Un umore, o il ſangue alterato da un veleno può produrre poco a poco negli animali a ſangue freddo dei ſconcerti anche maggiori di quello, che poſſano eſſer prodotti dalla circolazione arreſtata.

La morte, che ſegue immediatamente introdotto il veleno nel ſangue potrebbe far ſoſpettare, che vi è in quell' umore un principio piu attivo, piu ſottile, e piu volatile, che sfugge la viſta piu acuta ed il microſcopio medeſimo. Queſto principio parebbe in queſta ipoteſi neceſſario alla vita, e ſopra di queſto principio ſi crederebbe che il veleno portaſſe principalmente la ſua azione.

Che veramente eſiſta nèl ſangue un principio piu attivo e piu volatile, par che ſi poſſa ſoſpettare dal veder che il veleno della vipera impediſce il coagolo del ſangue cavato dai vaſi, e che per contrario lo produce dentro de vaſi medeſimi. Nel primo caſo ſi crederebbe, che è evaporata dal ſangue qualche coſa, che eſiſte nel ſangue dentro de' vaſi. In queſta ipoteſi queſto principio attivo, e di vita potrebbe conſiderarſi come il riſultato di tutta l'economia animale, nè i nervi anderebbono eſcluſi, che anzi potrebbono concorrervi il piu. Ma tutto queſto non è che ſemplice congettura piu, o meno probabile, e che l'eſperienza non dimoſtra. Biſogna tenerci ai fatti certi, qualunque poi ſia la maniera di ſpiegarli. Queſti fatti

2 ſono

fono, che il veleno Americano non agifce punto contro i nervi, e che agifce intieramente contro del fangue.

Neffuno avrebbe dubitato prima delle mie efperienze, che l'azione del veleno Americano non foffe immediatamente contro de' nervi. Tutti i fegni efterni la dichiavano tale. Quefti fegni fono adunque equivoci, e a torto fi prendono dai medici per prova ficura, che la malattià fia puramente nervofa. Vi poffono effere tutti quefti fegni fenza che i nervi fieno punto affetti: il folo fangue alterato bafta per farli nafcere nel momento. I piu gran medici hanno attribuito ad alterazione nervofa la malattià prodotta dal veleno della vipera, e dal veleno Americano; tocca ora ad effi medefimi di efaminare, fe altre malattie, che fi fono attribuite ai nervi non fono piuttofto malattie dei fluidi, malattie del fangue. Il fofpetto è grande, i fegni equivoci, il principio non dimoftrato nella fua generalità. Io non voglio già negare, che neffuna malattià poffa mai derivare dai nervi; quefto farebbe dare in un eftremo, per evitarne un altro. E indubitato, che molte malattie fono nervofe nella loro origine, e che molte altre lo fono per alterazione feguita in altre parti, anche femplicemente fluide; le paffioni dell' animo ci fanno vedere quel che poffano i nervi fopra le parti del corpo vivente. Ma tutto quefto non prova già, che tutte le malattie attribuite ai nervi fieno nervofe, e che i fegni ordi-

nari

nari di quelle malattie non sieno equivoci. Ed è poi certo che i veleni da noi esaminati non hanno alcuna azione immediata contro i nervi, come si è creduto comunemente fin qui. Si vorrà da tal uno objettare, che forse il veleno della vipera, e il veleno Americano non agiscono che sulle ultime extremità nervose, e che per questo sono innocenti quando si applicano ai tronchi nervosi. Ma cosa non si può mai objettare quando si vuol semplicemente objettare, e imaginar delle difficoltà? La piu piccola circostanza variata basta allora; e chi non sa trovar varietà quando è si difficile, che due cose sieno in tutto simili affatto? In quanto a me osservo che la sostanza interna dei nervi non si vede diversa da quello che è alle estremità di essi nervi, che il tronco è soggetto al dolore come lo sono le estremità, e che non immagino ipotesi, che i fatti non confermino.

Nella generalità delle illazioni, che deduco dalle mie esperienze posso essermi ingannato, e posso essermi ingannato ancora in qualcuna delle esperienze medesime, benchè abbia procurato di farle bene, ed abbia cercato la verità, senza prevenzione. Non dubito che chi vorrà applicarsi a queste ricerche dopo di me non trovi delle cose da aggiugnere, e forse ancora da correggere. A me basta di aver aperto una strada a nuove verità, e che i fatti principali che avanzo siano veri.

La

La piu gran parte di queste mie esperienze sono state fatte alla presenza di Mr. INGENHOUSZ, medico delle LL.MM. Cesaree, mio particolare amico, e uomo, che ha mostrato in piu opere il talento raro di osservatore. Il Sig. TIBERIO CAVALLO si è trovato presente anche egli a molte delle piu importanti. Ho creduto coll' autorità di due persone conosciute dai dotti di conciliare piu di credito alle mie esperienze.

Dopo aver finito le mie esperienze sopra il veleno Americano un mio amico a Londra mi ha procurato un gran numero di freccie dell' Indie orientali. Ho voluto fare qualche esperienza ancora sopra di esse, ma le mie esperienze non sono nè molte, nè variate, si perchè mi è parso, che questo veleno non fosse differente dall' altro, che per la minore attività, che mi ha dimostrato nell' uccidere gli animali. E questa minore attività probabilmente si deve attribuire, o perchè le freccie sono state piu mal conservate, che quelle dell' Indie occidentali, come pareva veramente, o perchè quel veleno era stato preparato molti anni prima.

Non mi è mai riuscito di far morire alcun cuniglio nè anco de' mezzani coll' applicarlo alla cute sfregata, o leggiermente tagliata. Benchè metessi di quel veleno in piu gran quantità, e sopra parti di pelle piu estese, che del veleno Ticunas, non produsse alcuna alterazione sensibile

sibile nè anco nei cunigli, che non pesavano, che appena una libbra.

Traforai colle freccie la pelle a piu animali, e ve la lasciai per giorni intieri, senza che potessi accorgermi, che gli animali fossero affetti del veleno; ma gli effetti del veleno furono bensì osservati, quando traforavo i muscoli colle freccie, e ve le lasciavo dentro di essi. Vari animali morirono in questa maniera, e morirono tutti con segni manifesti di veleno, e coi medesimi segni o sintomi, coi quali muojoni gli animali per il veleno Americano. E bensì vero, che nessuno morì, o mostrò di star male sensisibilmente, che dopo piu ore, talchè pare che questo veleno non differisca essenzialmente dall' altro, e conviene con esso affatto, quando si osserva col microscopio, quando si mescola col turnesole, quando si getta sugli occhi degli animali, e quando si assapora colla lingua, e si mastica fra denti; è per altro vero, che nell' acqua si scioglie men bene dell' altro veleno, che anzi la maggior parte resta insolubile a quel fluido. L'unica illazione, che si può dedurre dai fatti riportati sopra, è che il veleno comunicato ai muscoli è molto piu micidiale, che applicato alla pelle, che conviene molto bene agli altri veleni, e che sempre piu ci persuase, che l'azione immediata dei veleni non è contro dei nervi, giacchè

giacchè è certo, che la pelle è piu ſenſibile dei muſcoli, e che è tutta inteſſuta di nervi.

Alcune poche eſperienze feci ancora ſopra l'olio di tabacco, i riſultati delle quali ho creduto bene di dover quì accennare brevemente.

Eſperienze fatte coll' olio di tabacco.

Feci un piccolo taglio ſopra la coſcia deſtra di un Piccione, e vi applicai una goccia di olio di tabacco: dopo due minuti perdette il moto della zampa deſtra.

Ripetei la medeſima eſperienza ſopra di un altro Piccione, e l'eſito fù affatto il medeſimo.

Ferii con picciol taglio i muſcoli del petto d'un Piccione, e applicato alla ferita l'olio di tabacco, dopo trè minuti l'animale non poteva piu reggerſi ſulla zampa ſiniſtra.

Queſta medeſima eſperienza fu replicata ſopra di un altro Piccione, col medeſimo ſucceſſo.

Inſinuai nei muſcoli del petto d'un Piccione uno ſtecco inzuppato nell' olio di tabacco, e il Piccione, dopo pochi ſecondi cadde, come ſe foſſe ſtato morto.

Due altri Piccioni, ai muſcoli de quali avevo applicato l'olio di tabacco vomitorono piu volte tutto ciò che avevano mangiato.

Due altri trattati come sopra, ma a stomaco vuoto fecero sforzi per vomitare.

In generale ho osservato, che il vomito è l'effetto piu costante di questo olio, ma che la perdita del moto nella parte opposta del veleno non è che accidentale.

Nessuno poi degli animali, a cui applicai l'olio di tabacco morì.

Sopra l'acqua di Lauro Ceraso.

Finirò le mie esperienze sopra i veleni col riportarne alcune che ne ho fatte sopra un veleno divenuto celebre in Europa da qualche anno addietro. Questo veleno è l'acqua di Lauro Ceraso, che non la cede a nessun' altro dei piu attivi veleni, se si considera relativamente ai piu gravi sconcerti che apporta all' economia animale, e al breve tempo, in cui agisce quando si da per bocca agli animali. Egli produce non solo le piu forti convulsioni, e fino la morte, anche negli animali mediocremente grandi, ma di piu se viene dato in dose minore, l'animale si torce all' indietro accostando il capo alla coda, e inarca all' infuori talmente le vertebre, che fa orrore a vederlo in quello stato: le convulsioni, e i moti di tutto il corpo sono de piu violenti, e fra tanti sforzi muore alla fine l'animale dopo breve tempo. Se si da all' animale alla maniera di Clistere produce egualmente le convulsioni, e la morte. Con due soli cucchia-

ini da Tè di quell' acqua data per bocca, ho veduto dei cunigli di mediocre grandezza cader convulſi in meno di 30 ſecondi, e morire dentro un minuto. Se ſi da quell' acqua in gran quantità agli animali, muojono quaſi nell' iſtante, e muojono, ſenza convulſioni, colle parti rilaſciate, è cadenti.

Quando ſi da in poca quantità, le convulſioni ſono piu, o meno grandi, e le parti, che perdono prima delle altre il moto, ſono le zampe di dietro, e viene in appreſſo quelle davanti, che muojon piu tardi. Quando l'animale non muove piu le zampe, e il reſto del corpo, muove ancora beniſſimo il collo, e il capo, che ſeguita ad alzare con forza, e a volgere per tutto. In queſto ſtato l'animale ſente il ſuono, e vede gli oggetti; benchè non muova piu le zampe da per ſe, arriva per altro a muoverle, e a ritirarle quando ſi pungono forte, o ſi comprimano molto: ſegno, che può moverle, benchè non le muova, che per gran dolore.

L'acqua di Lauro Ceraſo è adunque un potentiſſimo veleno data per bocca, o introdotta nel corpo a foggia di Cliſtere. La ſua azione è ſi violenta, e ſi pronta, che ſi direbbe, che comincia ad agire nel momento, che l'animale la riceve per la bocca: certo è, che appena è entrata per l'eſofago nel ventricolo, che l'animale patiſce. E per altro vero, che una piccola doſe non fa nulla, cioè poche gocciole date ad un animale piccolo, che ſarebbe morto

se fosse stato del veleno Ticunas, non par che produca alcuno sconcerto sensibile. Ma tutto questo non fa una differenza essenziale fra quel veleno, e gli altri veleni piu conosciuti.

Io ho osservato, che mettendo una certa quantità di acqua nelle fogli di Lauro Ceraso si ottiene un liquore affatto innocente, se le foglie non sono moltissime, e se l'acqua non è in piccolissima dose. Se si distilla ancora piu volte successivamente quell' acqua sopra le medesime foglie, diventa è vero piu attiva, ma non per questo uccide ancora, ma se invece di unire al Lauro Ceraso dell' acqua si fa una distillazione a bagno maria, e si riceve l'umore distillato in questa maniera; egli è allora un potentissimo veleno, che uccide in brevissimo tempo. Di questo io ho fatto uso principalmente, ma non dubito punto, che non si potesse portare a tale attività da uccidere anche dato in piccola dose, come accade del veleno Americano. Basterebbe ridistillare piu volte sopra nuovo Lauro Ceraso bene asciutto, e quasi diseccato, il liquore sortito la prima volta: io credo che si otterrebbe alla fine sotto la forma d'una sostanza oleosa concreta, la quale se si facesse evaporare al fuoco, non solo non la cederebbe a nessuno dei veleni conosciuti, ma sarebbe superiore a tutti gli altri di gran lunga. Mi riservo di far questa esperienza in un altra occasione, nella quale parlerò ancora delle mandorle

 amare,

amare, e fino a qual grado di veleno ſi può portare la loro acqua diſtillata a ſecco.

L'acqua di Lauro Ceraſo uccide gli animali introdotta nelle cavità del corpo, ma quali effetti produce ella quando ſi applica alle ferite? Tra le diverſe eſperienze, che io feci, baſterà di accennarne qui una ſola. Aprii la pelle del baſſo ventre ad un cuniglio piuttoſto grande, e la ferita fù circa un pollice. Ferii legermente i ſotto-poſti muſcoli in piu luoghi, e vi inſinuai circa due, o tre chucchiajni da caffè di quell' acqua; in meno di tre minuti l'animale cadde in convulſione, e poco dopo morì. Queſta eſperienza ci fa vedere che l'acqua di Lauro Ceraſo è un veleno ſimile agli altri, e che agiſce quando s'introduce nel corpo, per mezzo di ferite. Queſta eſperienza ha avuto il medeſimo riſultato in altri animali a ſangue caldo, ma in tutti hò però trovato, che l'acqua di Lauro Ceraſo agiſce data per bocca con piu di forza, e piu preſto, data ancora in poca quantità; la qual coſa merita, a mio credere, la piu grande attenzione, perchè alla fine è una verità di fatto, che una gran ferita preſenta incomparabilmente piu di vaſi per aſſorbire quel veleno quaſi in momenti, che la bocca, il ventricolo, e le parti nervoſe ancora nelle ferite, per lo ſtato, in cui ſi trovano allora, devono ſentir piu facilmente l'azione di quel veleno. Nè ſolo gli animali a ſangue caldo muojono preſtiſſimo

quando

quando si fan bere di quell' acqua, ma gli stessi animale a sangue freddo muojono anche essi, e quello che mi è parso singulare è che muojono in brevissimo tempo, e forse anche piu presto, che è tutto il contrario negli altri veleni. Mi basterà ora di parlare delle anguille, animali difficilissimi a morire, e che morti ancora durano a muoversi per lungo tempo le loro parte. Questi animali muojono dopo pochi secondi che han bevuto di quell' acqua, e appena bevuta cominciano già a contrarsi, ma la morte che sopravviene subito gli rende immobili un momento dopo, nè urtate le loro parti si muovono piu. Il cuore però seguita ancora a muoversi, ma molto meno di prima, e finisce di muoversi molto prima, che quando si fanno morire tagliando loro il capo. Qui non si può negare, che l'irritabilità muscolare non sia estremamente affetta, e in modo particolare. Non so se vi sia nessuno animale a sangue freddo, che resista a questo veleno. Quelli, che ho provato sono tutti morti, e dubito se ve ne sia nessuno, a cui non sia veleno. Se è così egli merita una distinzione aparte anche per questo, e sarebbe il piu terribile di tutti i veleni conosciuti, anche per la sua generalità di dar la morte a qualunque sorta di animale. Ma come mai può egli uccidere in si breve tempo, quando si prende per bocca, e va al ventricolo, dove non si suppone vasi capaci di riceverlo?

La

La difficoltà domanda qualche esperienza ulteriore. Bisogna vedere quali effetti produce quando è applicato immediatamente ai nervi, e quali effetti produce quando è introdotto nel sangue, senza toccare a parti tagliate.

Mi sono servito dei cunigli piu grandi, ed ho fatte le mie esperienze sopra i nervi sciatici di quegli animali nella stessa maniera che ho fatto col veleno della vipera, e col veleno Americano. Mi basterà di accennare qui una sola esperienza, che servirà per tutte le altre, le quali per brevità tralascio, non le credendo molto necessarie, dopo le moltissime, che ho riportate sopra i nervi.

Avendo scoperto il nervo sciatico ad un grosso cuniglio per piu d'un pollice e mezzo, insinuai sotto il medesimo un inviluppo di tela finissima raddoppiata 16 volte, acciocché le parti sottoposte non fossero penetrate dall' acqua di Lauro Ceraso. Ferii allora il nervo di diversi colpi di lancetta fatti lungo il nervo medesimo e coprii tutto il tratto delle parti ferite, che era piu di otto linee, di un ammasso di cotone grosso circa 3 linee, e ben inzuppato dell' acqua di Lauro Ceraso. Piu di 15 gocciole di quell' acqua vi vollero per inumidir il cotone, e quest' acqua andava direttamente a comunicarsi per le ferite della lancetta alla sostanza midollare del nervo sciatico. Coprii ogni cosa dopo qualche minuto con nuovi cenci in modo, che era impossibile, che l'acqua del Lauro Ceraso

si

si comunicasse alle parti sottoposte, o vicine. Fatta la cucitura esterna, e lasciato in libertà l'animale non parve, che avesse sofferto nulla, ne in appresso mostrò di avere alcun male. Correva, mangiava, ed era così vivace, che prima: in somma questo animale non soffri nulla sensi-mente da quel veleno, che preso per bocca uccide si presto. Questo fatto è molto analogo, come molti altri, a quelli del veleno della vipera, e del veleno Americano, e ci fa vedere, che l'acqua di Lauro Ceraso applicata immediatamente sopra i nervi, e fino insinuata dentro la sostanza midollare di essi; non è veleno per nessun modo, onde, che non ha alcuna azione sopra i nervi comunque vi si applichi esternamente.

Dopo tante esperienze riportate nel decorso di questa memoria sopra il veleno della vipera, e sopra il veleno Americano ancor piu potente del primo, e dopo di aver veduto, che nè l'uno, nè l'altro di questi due veleni hanno azione alcuna sopra i nervi, quando vi si applica immediatamente, nel tempo che introdotti nel sangue uccidono immediatamente gli animali piu forti; nessuna altra cosa era piu naturale che di dedurre, che ancora il veleno del Lauro Ceraso il quale è innocente applicato egualmente che gli altri ai nervi, dovesse uccidere quando é introdotto nel sangue; eppure la cosa è affatto diversamente, tanto è vero, che bisogna diffidare dell' analogia anche allor

allor che pare piu uniforme. Io ho introdotto per le jugulari l'acqua di Lauro Ceraſo in un groſſo cuniglio, ne ho introdotto la prima volta cinque e piu gocciole nella medeſima maniera, che avevo introdotto il veleno della vipera, e il veleno Americano. L'animale non ha fatto alcun ſegno di ſoffrire; ho creduto di aver male operato, ho creduto di non avere introdotto nulla per quei vaſi, mi ſono immaginato, che la ſciringa ſi foſſe inſinuata per la cellulare: ho ripetuta l'eſperienza, ho introdotto di nuovo per la jugulare una quantità di veleno, forſe 3, in 4 volte maggiore, mi ſono aſſicurato prima d'introdurre il veleno, che la punta della mia ſciringa entrava nella jugulare, e che il veleno non poteva tornare a dietro per neſſun conto, ma l'animale non ha moſtrato di ſoffrir nulla per queſto, ed era dopo coſi vivace che prima. Io ero piutoſto maravigliato di tutto queſto, che ſodisfatto. Non ſapevo perſuadermi che l'acqua di Lauro Ceraſo non doveſſe eſſer veleno, e un veleno potentiſſimo appena introdotta nel ſangue, quando era veleno applicata alle carni ferite, e preſa per bocca, e nel' tempo ſteſſo inattiva e innocente meſſa ſopra i nervi. Ripetei adunque le eſperienze, e introduſſi queſta volta per le jugulari un intiero cucchiaio da Tè di acqua di Lauro Ceraſo: l'animale non ſoffrì nulla, ed era tanto ſano, che prima. Replicai queſta eſperienza in un altro

 cuniglio,

cuniglio, a cui pure introdussi per le jugulari un buon cucchiajo da Tè di questo stesso veleno: il cuniglio non dette alcun segno di soffrire nè allora, nè dopo.

L'esito inaspettato di queste esperienze mi getta nella piu grande incertezza intorno all' azione di quel veleno, nè so intendere non solo in quel maniera opera, ma nè anco sopra quali parti agisca, quando si prende per bocca, o si applica alle ferite. Qui tutto si confonde. Non si vede che agisca su i nervi, non ha azione alcuna sul sangue, eppure uccide, e uccide in istanti, se si introduce per la bocca nel ventricolo.

Vi è egli dunque una nuova via per introdursi la morte negli animale oltre quella del sangue, e dei nervi? Il moto perduto, e perduto in pochi secondi in animali, come sono le anguille, che seguitano a muoversi per ore dopo recisa la testa, e dopo tagliate in pezzi, farebbe credere, che l'irritabilità della fibra muscolare fosse affetta da quel veleno. E vero, che il cuore seguita ancora a muoversi in quegli animali, ma il moto nè moltissimo diminuito, ed è di breve durata. Negli animäli caldi morti per quel veleno sussiste ancora il moto, benchè pochissimo, e se il cuore in essi dura a battere per qualche tempo, batte meno forte, che quando si fanno morire in altre maniere. L'irritabilita è sicuramente diminuita moltissimo in molti animali, e in molti altri affatto dis-

tratta,

tratta, comunque poi essa possa contribuire alla morte, e uccidere in si breve tempo, e comunque sia oscura il meccanismo, per cui la fibra muscolare perde la sua irritabilità. Bisogna confessare la nostra ignoranza nelle ricerche della natura. Quando crediamo di aver tutto fatto ci troviamo spesso ritornati d'onde siamo partite. L'esperienza è la sola guida, che abbiamo nelle nostre ricerche; l'esperienza, è vero, è un mezzo sicuro, per non cader nell' errore, ma l'esperienza non sempre ci porta alle verità piu remote, non sempre ci guida alla conoscenza dei nascosti arcani della natura, nè sempre ci conduce, dove ci eramo proposti di andare.

Ma se noi non sappiamo, come operi l'acqua di Lauro Ceraso, o per meglio dire sopra quali parti quel veleno eserciti la sua azione, quando uccide gli animali, sappiamo però, che applicato immediatamente ai nervi, e fino alla parte midollare di essi è affatto innocente, e non è men vero tutto quello che tante esperienze riportate sopra ci hanno dimostrato chiaramente, che il veleno della vipera, e il veleno Americano non sono veleno, applicati comunque ai nervi, ma che lo sono allora che sono introdotti nel sangue. Questi sono fatti prima ignoti, son verità ora, nè si possono rivocare in dubbio da chi che sia. Questi fatti distruggono tutti i sistemi inventati dagli scrittori sopra l'azione di quei veleni, e da questi fatti dobbiamo

partire per l'intelligenza di quei veleni, e della loro azione.

Qualche lume avrei probabilmente potuto cavare sopra l'azione del veleno di Lauro Ceraso, se lo avessi potuto applicare alle diverse parti del cervello nell' animale vivente, ma mi riservo di farlo quando avrò piu comodo che al presente, e quando avrò ridotto a consistenza di sciroppo l'acqua di Lauro Ceraso. In quello stato reso assai piu attivo quel veleno potrà facilmente presentarmi dei fatti nuovi, e piu interessanti, e potrà forse darmi dei lumi meno equivoci sopra la sua azione, e farmi giudicare sopra quali parti dell' animale vivente agisce per uccidere. In questa medesima occasione io mi riserverò di esaminare se quel veleno agisce sopra i vasi linfatici, o per meglio dire sopra la linfa medesima. Questo è un semplice sospetto, che mi è venuto dopo, e che le presenti mie circostanze non mi permettano per ora di esaminare. Sono adunque sforzato di dare le mie esperienze sopra questo soggetto in parte mancanti, e difettose. Andavano piu moltiplicate, piu seguite, che non ho potuto fare; ed è questa appunto una ragione di piu, perchè io ritorni a trattar di nuovo questa materia, che non lascia di essere interessante.

XII. *A Conjecture concerning the Method by which* Cardan's *Rules for resolving the Cubic Equation* $x^3+qx=r$ *in all cases (or in all magnitudes of the known quantities q and r) and the Cubic Equation* $x^3-qx=r$ *in the first Case of it (or when r is greater than* $\frac{2q\sqrt{q}}{3\sqrt{3}}$, *or* $\frac{rr}{4}$ *is greater than* $\frac{q^3}{27}$*) were probably discovered by* Scipio Ferreus, *of* Bononia, *or whoever else was the first Inventor of them. By* Francis Maseres, *Esq. F. R. S. Cursitor Baron of the Exchequer.*

Read January 27, 1780.

ARTICLE I.

THERE is nothing more amusing, or more grateful to an inquisitive mind, in the study of the sciences of Geometry and Algebra (for if we banish from it the ridiculous mysteries arising from the supposition of *negative* quantities, or quantities *less than nothing*, the latter may deserve the name of a *science* as well as the former) than to contemplate the methods by which the several ingenious and surprizing truths that are delivered in the books

books that treat of them were firſt diſcovered. This we are ſometimes enabled to do by the authors themſelves to whom we are indebted for theſe diſcoveries, who have candidly informed their readers of the ſeveral ſteps, and ſometimes of the accidents, by which they have been led to them: but it alſo often happens, that the authors of theſe diſcoveries have neglected to give their readers this ſatiſfaction, and have contented themſelves with either barely delivering the propoſitions they have found out, without any demonſtrations, or with giving formal and poſitive demonſtrations of them, which command indeed the aſſent of the underſtanding to their truth, but afford no clue whereby to diſcover the train of reaſoning by which they were firſt found out; and conſequently contribute but little to enable the reader to make ſimilar diſcoveries himſelf on the like ſubjects. This ſeems to be the caſe with thoſe ingenious rules for the reſolution of certain cubic equations, which are uſually known by the name of CARDAN's rules. We are told to make certain ſubſtitutions of ſome quantities for others in theſe equations $x^3+qx=r$ and $x^3-qx=r$ (which are the objects of thoſe rules) and certain ſuppoſitions concerning the quantities ſo ſubſtituted; by doing which we find, that thoſe equations will be transformed into other equations which will involve the ſixth power of the unknown quantity

tity contained in them, but which (though of double the dimenſions of the original equations $x^3+qx=r$ and $x^3-qx=r$, from which they were derived) will be more eaſy to reſolve than thoſe equations, becauſe they will contain only the ſixth power and the cube of the unknown quantity which is their root, and conſequently will be of the ſame form as quadratic equations; ſo that by reſolving them as quadratic equations we may obtain the value of the cube of the unknown quantity which is their root, and afterwards, by extracting the cube-root of the ſaid value, we may obtain the value of the ſaid root, or unknown quantity, itſelf; and then at laſt, by the relation of this laſt root to x, or the root of the original equation, (which relation is derived from the ſuppoſitions that have been made in the courſe of the preceding tranſformations) we may determine the value of x. And, if we pleaſe to examine the ſeveral ſteps of this proceſs with ſufficient attention, we may perceive, as we go along, that all theſe ſubſtitutions are legitimate and practicable, or are founded upon poſſible ſuppoſitions; though I cannot but obſerve, that the writers on algebra, for the moſt part, have not been ſo kind as to ſhew us that they are ſo. But ſtill the queſtion recurs, "How came SCIPIO "FERREUS, of Bononia (who, as CARDAN tells us, was "the firſt inventor of theſe rules) or the other perſon, "whoever

" whoever he was, that invented them, to think of mak-
" ing theſe lucky ſubſtitutions which thus transform the
" original cubic equations into equations of the ſixth
" power which contain only the ſixth and third powers
" of the unknown quantities which are their roots, and
" conſequently are of the form of quadratic equations?"
To anſwer this queſtion as well as I can *by conjecture* (for I know of no hiſtorical account of this matter in any book of algebra) and in a manner that appears to me to be *probable*, is the deſign of the following pages.

2. The moſt probable conjecture concerning the invention of theſe rules, called CARDAN's rules, by SCIPIO FERREUS, of Bononia, or whoever elſe was the inventor of them, ſeems to be this: that the ſaid inventor tried a great variety of methods of reducing the three cubic equations of the third claſs, to wit, $x^3+qx=r$ and $x^3-qx=r$, and $qx-x^3=r$ (to ſome one of which all other cubic equations may, by proper ſubſtitutions, be reduced) to a lower degree, or to a more ſimple form, by ſubſtituting various quantities in the ſtead of x, in hopes that ſome of the terms ariſing by ſuch ſubſtitutions might be equal to others of them, and, having contrary ſigns prefixed to them, might deſtroy them, and thereby render the new equation more ſimple and manageable than the old one. And, amongſt other trials, it ſeems natural to

imagine, that he would ſubſtitute the ſum or difference of two other quantities inſtead of x, as being the moſt ſimple and obvious ſubſtitutions that could be made. And by making theſe ſubſtitutions, the above mentioned rules would of courſe come to be diſcovered, as well as the aforeſaid limitation of them in the reſolution of the equation $x^3-qx=r$, which reſtrains the rule to thoſe caſes only in which r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, and their utter inutility in all the caſes of the equation $qx-x^3=r$. This will appear by examining each of theſe equations ſeparately in the following manner.

Of the equation $x^3+qx=r$.

ART. 3. In the equation $x^3+qx=r$ the inveſtigator of theſe rules would naturally be inclined to ſubſtitute the difference of two quantities (which we will here call y and z, and of which we will ſuppoſe y to be the greater) inſtead of x, rather than their ſum, or would ſuppoſe x to be equal to $y-z$, rather than to $y+z$; becauſe, if he was to ſuppoſe x to be equal to the ſum of the two quantities y and z, and was to ſubſtitute that ſum, or the binomial quantity $y+z$, inſtead of x in the equation $x^3+qx=r$, it is evident, that (as the ſigns of x^3 and qx are,

are, both of them, affirmative) the terms of the new equation, arising from such substitution, would all of them be likewise affirmative; and consequently none of them, though they should happen to be exactly equal to each other, could exterminate each other, and thereby render the new equation more simple than the old one, which was the only view with which the substitution would have been made. He would, therefore, suppose x to be equal to $y-z$; and by substituting this quantity instead of x in the original equation $x^3+qx=r$, he would transform that equation into the following one, to wit,

$$y^3-3yyz+3yzz-z^3+qy-qz=r,$$

$$\text{or } y^3-3yz\times\overline{y-z}-z^3+q\times\overline{y-z}=r.$$

Now in this equation it is evident, that the terms $3yz\times\overline{y-z}$ and $q\times\overline{y-z}$ have contrary signs; and therefore, if their co-efficients $3yz$ and q can be supposed to be equal to each other, those terms will mutually destroy each other, and the equation will be reduced to the following short one, $y^3-z^3=r$. And if in this equation we substitute, instead of z, its value $\frac{q}{3y}$, derived from the same supposition of the equality of q and $3yz$, the equation will be $y^3-\frac{q^3}{27y^3}=r$; and, by multiplying both sides by y^3, it will be $y^6-\frac{q^3}{27}=ry^3$; which equation, though it rises to the sixth power of the unknown quantity y, is

4 evidently

evidently of the form of a quadratic equation, and may therefore be resolved, so far as to find the value of the cube of y, in the same manner as a quadratic equation; after which it will be possible to find the value of y itself by the mere extraction of the cube root; and then at last, from the relation of y to x (derived from the foregoing suppositions that $y-z$ was equal to x, and that $3yz$ was equal to q, and consequently z equal to $\frac{q}{3y}$) we shall be able to determine the value of x.

Art. 4. It would therefore remain for the investigator of this method to inquire, whether or no the supposition, "that $3yz$ was equal to q," was a possible supposition; that is, whether it was possible (whatever might be the magnitudes of q and r) for two quantities, y and z, to exist, whose nature should be such that their difference $y-z$ should be equal to the unknown quantity x in the equation $x^3+qx=r$, and that three times their product should at the same time be equal to q, or their simple product to the third part of q. And this supposition he would soon find to be always possible, whatever may be the magnitudes of q and r; because, if the lesser quantity z is supposed to increase from *o ad infinitum*, and the greater quantity y is likewise supposed to increase with equal swiftness, or to receive equal increments in the same

 times,

times, and thereby to preserve their difference $y-z$ always of the same magnitude, or equal to x, it is evident that the product or rectangle yz will increase continually at the same time from o *ad infinitum*, and consequently will pass successively through all degrees of magnitude, and therefore must at one point of time during its increase become equal to $\frac{q}{3}$.

And having thus found this supposition of the equality of yz and $\frac{q}{3}$, or of $3yz$ and q, to be always possible, whatever might be the magnitudes of q and r; our investigator would justly consider his solution of the equation $x^3+qx=r$ (which was founded on that supposition) as legitimate and compleat. And thus we see in what manner it seems probable, that CARDAN's rule for resolving the cubic equation $x^3+qx=r$ may have been discovered.

Of the equation $x^3-qx=r$.

Art. 5. In this second equation $x^3-qx=r$, in which the second term qx is subtracted from the first, or marked with the sign $-$, it seems to have been natural for the person who invented these rules to substitute the *sum* as well as the *difference* of two other quantities, y and z, instead

ftead of x, in the terms x^3 and qx, in hopes of fuch an extermination of equal terms, and confequential reduction of the equation to one of a fimpler and more manageable form, as was found to be fo ufeful in the cafe of the former equation $x^3+qx=r$. We will therefore try both thefe fubftitutions; and, as that of the difference $y-z$ has in the former cafe proved fo fuccefsful, we will begin by that.

Art. 6. Now, by fubftituting the difference $y-z$ inftead of x in the equation $x^3-qx=r$, we fhall transform it into the following equation, to wit, $y^3-3yyz+3yzz-z^3-q \times \overline{y-z}=r$, or $y^3-3yz\times\overline{y-z}-z^3-q\times\overline{y-z}=r$; in which the terms $3yz\times\overline{y-z}$ and $q\times\overline{y-z}$ have both of them the fame fign $-$ prefixed to them, and confequently can never exterminate each other, whether $3yz$ be equal or unequal to q. This fubftitution therefore is in this cafe of no ufe.

Art. 7. We will now therefore try the fubftitution of the *fum* of y and z, inftead of their difference, in the equation $x^3-qx=r$.

Now, if x be fuppofed to be equal to $y+z$, and $y+z$ be fubftituted inftead of it in the equation $x^3-qx=r$, that equation will be thereby transformed into the following one, to wit,

y^3+

$$y^3+3yyz+3yzz+z^3-q\times\overline{y+z}=r,$$

$$\text{or } y^3+3yz\times\overline{y+z}+z^3-q\times\overline{y+z}=r.$$

Now in this equation, the terms $3yz\times\overline{y+z}$ and $q\times\overline{y+z}$ have contrary ſigns. Conſequently, if they can be ſuppoſed to be equal to each other, they will deſtroy each other, and the equation will be thereby reduced to the following ſhort one, $y^3+z^3=r$; that is, if $3yz$ and q can be ſuppoſed to be equal to each other, or if yz can be ſuppoſed to be equal to $\frac{q}{3}$, the equation will be reduced to the ſhort equation $y^3+z^3=r$. And, if in this ſhort equation we ſubſtitute, inſtead of z, its value $\frac{q}{3y}$ (derived from the ſame ſuppoſition of the equality of $3yz$ and q) the equation thence reſulting will be $y^3+\frac{q^3}{27y^3}=r$; and by multiplying both ſides by y^3, it will be $y^6+\frac{q^3}{27}=ry^3$; and, by ſubtracting y^6 from both ſides, it will be $ry^3-y^6=\frac{q^3}{27}$; which, though it riſes to the ſixth power of y, is evidently of the form of a quadratic equation, and conſequently may be reſolved in the ſame manner as a quadratic equation, ſo far as to find the value of y^3, or the cube of the root y; after which it will be poſſible to find the value of y itſelf by the mere extraction of the cube root; and, laſtly, from the relation of y to x (contained in the two ſuppoſitions, that $y+z$ is equal to x, and that

3yz

$3yz$ is equal to q, and consequently that z is equal to $\frac{q}{3y}$) we may determine the value of x.

Art. 8. The only thing, therefore, that would remain for the investigator of these rules to do, in order to know whether the foregoing method of resolving the equation $x^3+qx=r$ was practicable or not, would be to inquire, whether it was possible in all cases, that is, in all magnitudes of the known quantities q and r, for $3yz$ to be equal to q, or for yz (or the product or rectangle of the two quantities y and z, whose sum is equal to x) to be equal to $\frac{q}{3}$, and, if it was not possible in all cases, but only in some, to determine in what cases it *was* possible, or what must be the relation between q and r to make it possible.

Art. 9. Now, in order to determine this question, it would be proper and natural to observe, that the quantity yz, or the product of the two quantities y and z, whose sum is supposed to be equal to x, can never be greater than the square of half that sum, that is, than the square of $\frac{x}{2}$, or than $\frac{xx}{4}$, by El. 2, 5, but may be of any magnitude that does not exceed that square. Therefore, if $\frac{q}{3}$ is greater than $\frac{xx}{4}$, it will be impossible for yz to be equal to it; but, if $\frac{q}{3}$ is either equal to, or less than,

$\frac{xx}{4}$,

$\frac{xx}{4}$, it will be possible for yz to be equal to it, and if $\frac{q}{3}$ is exactly equal to $\frac{xx}{4}$, z will be exactly equal to y, and each of them equal to one half of x. We must, therefore, inquire what is the magnitude of x when $\frac{q}{3}$ is equal to $\frac{xx}{4}$. Now, when $\frac{xx}{4}$ is $= \frac{q}{3}$, xx will be $= \frac{4q}{3}$, and $x = \frac{2\sqrt{q}}{\sqrt{3}}$: therefore, when x is less than $\frac{2\sqrt{q}}{\sqrt{3}}$, it will be impossible for yz to be equal to $\frac{q}{3}$; but when x is greater than $\frac{2\sqrt{q}}{\sqrt{3}}$, it will be possible for yz to be equal to $\frac{q}{3}$.

But when x is $= \frac{2\sqrt{q}}{\sqrt{3}}$, x^3 will be $= \frac{8q\sqrt{q}}{3\sqrt{3}}$, and qx will be $= \frac{2q\sqrt{q}}{\sqrt{3}}$ or $\frac{6q\sqrt{q}}{3\sqrt{3}}$, and consequently $x^3 - qx$ will be $= \frac{8q\sqrt{q}}{3\sqrt{3}} - \frac{6q\sqrt{q}}{3\sqrt{3}} = \frac{2q\sqrt{q}}{3\sqrt{3}}$.

Therefore, if it be true (as we shall presently see that it is) that while x increases from being equal to $\sqrt{q}$ (which is evidently its least possible magnitude) to any other magnitude, the compound quantity $x^3 - qx$, or the excess of x^3 above qx, will also continually increase from o (to which it is equal when x is $= \sqrt{q}$, or xx is $= q$) to some correspondent magnitude without ever decreasing; it will follow that, when x is less than $\frac{2\sqrt{q}}{\sqrt{3}}$, the compound quantity $x^3 - qx$ will be less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$; and when

x is

x is greater than $\frac{2\sqrt{q}}{\sqrt{q}}$, the compound quantity x^3-qx will be greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$; and, *è converso*, if the compound quantity x^3-qx is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, x will be less than $\frac{2\sqrt{q}}{\sqrt{3}}$; and, if the compound quantity x^3-qx is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, x will be greater than $\frac{2\sqrt{q}}{\sqrt{3}}$. Consequently, if the compound quantity x^3-qx, or, its equal, the absolute term r in the equation $x^3-qx=r$, is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, it will be impossible for yz to be equal to $\frac{q}{3}$; but, if x^3-qx, or r, is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, it will be possible for yz to be equal to $\frac{q}{3}$. Therefore, if x^3-qx, or r, is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, the foregoing method of resolving the cubic equation $x^3-qx=r$ will be impracticable; but, if $x^3-qx=r$, or r, is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, it will be practicable.

Art. 10. It now only remains to be proved, that while x increases, from being equal to $\sqrt{q}$, *ad infinitum*, the compound quantity x^3-qx will likewise increase from o *ad infinitum*, without ever decreasing. Now this may be demonstrated as follows.

Art. 11. It is evident, that while x increafes from being equal to $\sqrt{q}$ *ad infinitum*, both the quantities x^3 and qx will increafe *ad infinitum* likewife. But it does not therefore follow, that the excefs of x^3 above qx will continually increafe at the fame time. This will depend upon the relation of the contemporary increments of x^3 and qx: if the increment of x^3 in any given time is equal to the contemporary increment of qx, the compound quantity x^3-qx will neither increafe nor decreafe, but continue always of the fame magnitude during the faid time, notwithftanding the increafe of x; if the former increment is lefs than the latter, the faid compound quantity will decreafe; and, if it is greater, it will increafe. We muft therefore inquire, whether the increment of x^3 in any given time is greater or lefs than the contemporary increment of qx.

Art. 12. Now, if $\dot{x}$ be put for the increment which x receives in any given time, the increment of x^3 in the fame time will be the excefs of $\overline{x+\dot{x}}|^3$ above x^3, that is, the excefs of $x^3+3x^2\dot{x}+3x\dot{x}^2+\dot{x}^3$ above x^3; and the increment of qx in the fame time will be the excefs of $q\times\overline{x+\dot{x}}$, or $qx+q\dot{x}$, above qx; that is, the increment of x^3 will be $3x^2\dot{x}+3x\dot{x}^2+\dot{x}^3$, and that of qx will be $q\dot{x}$. Now in the equation $x^3-qx=r$ it is evident, that xx muft be greater than q; for otherwife x^3 would not be greater than qx, as

as it is supposed to be. Consequently, $xx \times \dot{x}$ must be greater than $q\dot{x}$; and, *à fortiori*, $3x^2\dot{x}+3x\dot{x}^2+\dot{x}^3$ (which is more than triple of $x^2\dot{x}$) must be greater than $q\dot{x}$; that is, the increment of x^3 will be greater than the contemporary increment of qx. Therefore, the excess of x^3 above qx, or the compound quantity x^3-qx, will increase continually, without decreasing, while x increases from $\sqrt{q}$ *ad infinitum*. Q. E. D.

Art. 13. It follows, therefore, upon the whole of these inquiries, that if the compound quantity x^3-qx, or, its equal, the absolute term r, is less than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is less than $\frac{q^3}{27}$, it will be impossible for yz to be equal to $\frac{q}{3}$, and consequently the foregoing method of resolving the equation $x^3-qx=r$ will be impracticable; but, if x^3-qx or r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, it will be possible for yz to be equal to $\frac{q}{3}$, and consequently, the foregoing method of resolving the equation $x^3-qx=r$ will be practicable. And thus we see in what manner it is probable that CARDAN's rule for resolving the cubic equation $x^3-qx=r$ in the first case of it, or when r is greater than $\frac{2q\sqrt{q}}{3\sqrt{3}}$, or $\frac{rr}{4}$ is greater than $\frac{q^3}{27}$, together with the restriction of it to that first case, may have been discovered.

Of the Equation $qx-x^3=r$.

Art. 14. In the third equation $qx-x^3=r$ the terms x^3 and qx have different ſigns, as well as in the ſecond equation $x^3-qx=r$; and therefore it ſeems to have been natural for the inventor of CARDAN's rules to try both the ſubſtitutions of $y-z$ and $y+z$ inſtead of x in this equation, as well as in that ſecond equation, in hopes of an extermination of equal terms that are marked with contrary ſigns, and a conſequent reduction of the equation to another which, though of double the dimenſions of the equation $qx-x^3=r$, ſhould have been of a ſimpler form and more eaſy to be reſolved. But it will be found upon trial, that neither of theſe ſubſtitutions will anſwer the end propoſed.

Art. 15. For, in the firſt place, let us ſuppoſe x to be $=y-z$. Then we ſhall have $x^3=y^3-3yyz+3yzz-z^3=y^3-3yz\times\overline{y-z}-z^3$, and $qx=q\times\overline{y-z}$, and conſequently $qx-x^3=q\times\overline{y-z}-y^3+3yz\times\overline{y-z}+z^3$. Therefore, $q\times\overline{y-z}-y^3+3yz\times\overline{y-z}+z^3$ will be $=r$. Now in this equation it is evident, the terms $q\times\overline{y-z}$ and $3yz\times\overline{y-z}$ have the ſame ſigns, and therefore can never deſtroy each other. Therefore, no ſuch method of reſolving this equation

$qx-x^3=r$ as was found above for the two former equations $x^3+qx=r$ and $x^3-qx=r$, can be obtained by ſubſtituting the difference $y-z$ in it inſtead of x.

Art. 16. We will now try the ſubſtitution of $y+z$ inſtead of x in the terms of this equation.

Now, if x be ſuppoſed to be $=y+z$, we ſhall have $x^3=y^3+3yyz+3yzz+z^3=y^3+3yz\times\overline{y+z}+z^3$, and $qx=q\times\overline{y+z}$, and conſequently, $qx-x^3=q\times\overline{y+z}-y^3-3yz\times\overline{y+z}-z^3$. Therefore, $q\times\overline{y+z}-y^3-3yz\times\overline{y+z}-z^3$ will be $=r$.

In this equation it is true indeed that the terms $q\times\overline{y+z}$ and $3yz\times\overline{y-z}$ have different ſigns. But they cannot be equal to each other: for, ſince the three terms y^3 and $3yz\times\overline{y-z}$ and z^3 are all marked with the ſign $-$, or are to be ſubtracted from the firſt term $q\times\overline{y+z}$, and the remainder is $=r$, it is evident, that $q\times\overline{y+z}$ muſt be greater than the ſum of all the three terms y^3, $3yz\times\overline{y+z}$, and z^3, taken together, and therefore, *à fortiori*, greater than $3yz\times\overline{y+z}$ alone. Therefore, no ſuch extermination of equal terms marked with contrary ſigns as took place in the transformed equations derived from the two former equations $x^3+qx=r$ and $x^3-qx=r$, can take place in this transformed equation derived from the equation $qx-x^3=r$ by ſubſtituting $y+z$ in its terms inſtead of x; and conſequently no ſuch method of reſolving the equation

tion $qx-x^3=r$ as has been found for the reſolution of the equations $x^3+qx=r$ and $x^3-qx=r$, can be obtained by means of that ſubſtitution.

Art. 17. Theſe are the methods of inveſtigation by which I conceive it to be probable, that CARDAN's rules for the reſolution of the cubic equations $x^3+qx=r$ and $x^3-qx=r$, together with the limitation of the rule relating to the latter of thoſe equations, and their inapplicability to the third equation $qx-x^3=r$, may have been diſcovered by the firſt inventor of them.

XIII. *A new Method of treating the* Fiſtula Lachrymalis. *By Mr.* William Blizard, *Surgeon, F. A. S.*; *communicated by Mr.* Joſeph Warner, *Surgeon, F. R. S.*

Read Feb. 24, 1780.

IN every period of the diſeaſe, termed *fiſtula lachrymalis*, there is underſtood to exiſt a degree of obſtruction in the naſal duct; ſo that more or leſs of the tears, mixed with the oily ſecretion of the ſebaceous glands of the eye-lids, and mucus of the internal ſurface of the lachrymal ſac, being prevented from paſſing into the noſe, are expelled through the lachrymal puncta upon the ſurface of the eye, and down the cheek.

Writers on ſurgery divide this diſeaſe into ſeveral ſtages; the firſt and moſt ſimple being that of obſtruction, with little or no inflammation; and ſo on, according to the degree or effect of inflammation, to the laſt ſtage, a ſloughy, ulcerated condition of the ſac and its integuments, with, now and then, a *caries* of the bony parts.

Though

Though the diſeaſe be frequently the effect of a *virus* in the habit, yet ſurgeons find, that ſometimes the cauſe is very ſimple, and eaſily to be conceived from the analogy of parts.

The membranous portions of the naſal duct and lachrymal ſac are a continuation of the pituitous membrane of the noſe. This membrane is exceedingly vaſcular, ſecretes a large quantity of mucus upon its internal ſurface, and is endued with a great degree of ſenſibility.

Experience ſhews the great defluxions that are oftentimes made upon the pituitous membrane; the increaſed ſecretion of mucus that happens upon the application of various ſtimulants; and the firm conſiſtence it often acquires from ſtagnation, abſorption, and evaporation of its thinner parts: moreover, that the membrane itſelf frequently becomes inflamed and thickened.

The duct and ſac may be affected through obſtructed perſpiration, &c.; and thickened from the turgid ſtate of their veſſels: the ſecretion of mucus may alſo be conſiderably augmented. From the thickened ſtate of the membrane of the duct, the fluids in the ſac paſs with difficulty: by retention, warmth, and abſorption, they are rendered viſcid; and the difficulty, that at firſt aroſe from the thickened ſtate of the membrane,

now ariſes from another cauſe, namely, the inſpiſſated ſtate of the fluids.

Theſe are, probably, the moſt ſimple cauſes of obſtruction in the naſal duct; but, from whatever cauſe the obſtruction had its origin, in its early ſtate, when unattended with a morbid change of the contiguous parts, it is conſidered as the firſt and moſt ſimple ſtage of the *fiſtula lachrymalis*. It is in this ſtage that the means of obviating the neceſſity of a troubleſome and uncertain operation ſhould be employed, with any rational expectation of ſucceſs.

The principal of theſe means are:

1. Compreſſion; declared by experienced practitioners to be injudicious.

2. The paſſing an inſtrument into the noſtril, and up the duct; an operation very painful to the patient, and exceedingly troubleſome to the operator.

3. The introducing a probe through one of the puncta into the duct, after M. ANEL's manner; by experience proved to be inadequate to the deſign.

4. The impelling a fluid, by a ſyringe, through one of the puncta, as directed by M. ANEL; allowed by judicious and experienced ſurgeons to be ſometimes uſeful.

On reflecting upon the laſt method, I was induced to think, that if a fluid, of a great degree of ſpecific

gravity, as quickſilver, could be paſſed through one of the puncta, ſo as to fill the ſac and duct, and preſs upon the obſtructed part, it might be reaſonably expected to remove the obſtruction in the firſt and ſimple ſtage of the diſeaſe; at leaſt, to have a much better chance of producing this effect than a watery fluid, urged through the punctum in an unfavourable direction: beſides, it would be no bar to the uſe of proper general means.

Flattered with the ſeeming reaſonableneſs of the ſuggeſtion, and convinced of the ſafety of the experiment, I reſolved on making a trial the firſt opportunity; which ſoon occurred to me.

Mr. M—— B——, a ſadler, in Mark-Lane, had been troubled with a flux of tears and mucus down the cheek from the puncta of the right eye-lids, about ſeven months. There was a degree of ſwelling or diſtenſion of the ſac, attended with pain. Upon preſſing the ſac, much ropy fluid, of a whitiſh colour, was forced through the puncta. The diſcharge was always in greateſt abundance in the evening; at which time he had a dimneſs of ſight in that eye.

The uſual means had been employed, without ſucceſs, by his ſurgeon, who approved of the ſuggeſted experiment, and the patient agreed to have it tried.

Meſſ. NAIRNE and BLUNT provided an inſtrument for the purpoſe. It conſiſts of a fine ſteel pipe, a little curved, cemented

cemented in a glaſs tube about ſix inches long. At the top of the tube is a wooden funnel; and at the bottom of this is a valve, which may be elevated by a ſilken ſtring that is conveyed through a hole in the brim of the funnel, and hangs down by the ſide of the tube[(a)].

The ſteel pipe was paſſed into the inferior punctum, without pain or difficulty. The quickſilver was then poured into the funnel, and let down the tube by pulling the ſtring of the valve. When the quickſilver regurgitated out by the ſuperior punctum, the inſtrument was withdrawn. The quickſilver lay in the ſac and duct, without exciting pain, about thirty hours, when it paſſed into the noſe, and the patient caught ſome of it in his hand.

I thought it beſt at this time not to compreſs the ſac; apprehending it would diſcharge the quickſilver through the puncta, and ſo fruſtrate the intention.

On the third day the operation was repeated; when, on gently compreſſing the ſac, ſome of the quickſilver paſſed into the noſe, and with it a piece of congealed whitiſh mucus. A ſmall quantity of the quickſilver, upon making the preſſure, returned through the puncta.

(*a*) I have deſcribed the inſtrument as it was uſed; but I have ſince thought, that it would not only be more ſimple but do as well without a valvular apparatus, the quickſilver being poured in by an aſſiſtant. (See the figure).

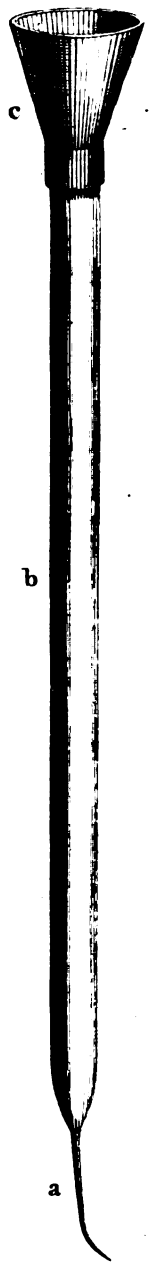
At the third and fourth times of repeating the operation, without any compreffion, at intervals of a few days, the quickfilver paffed readily into the nofe.

I once introduced the point of a fteel pipe, ufed for injecting the lymphatic veffels. It is cemented to a tube of glafs eighteen inches long. This pipe is not fo fine as that of the other inftrument, yet it was conveyed into the punctum without difficulty, and with little or no pain. To gain a greater degree of *momentum* I raifed the column of quickfilver to about twelve inches, when it flowed into the nofe with a confiderable degree of velocity.

From the time that the quickfilver paffed into the nofe, lefs fluid trickled down the cheek than before. After the fecond or third operation, the fwelling or diftention of the fac intirely fubfided. The patient at this time has no difcharge of mucus, and a tear but very feldom: the parts have a perfectly healthy appearance.

To afcertain the effects of medicines in difeafes of the conftitution, many experiments, under various circumftances, are neceffary; but in matters determinable by a mechanical operation, the effect, as far as our fenfes can direct us, is in general very plain and explicable.

In the cafe related this is clear, namely, that previoufly to the injecting of quickfilver, the tears, febaceous matter,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ter, and mucus, did not paſs through the naſal duct, or, but in a very ſmall proportion to the quantity ſecreted; that at the firſt experiment, quickſilver did not paſs; but that quickſilver, tears, &c., have ſince readily paſſed.

I cannot, however, flatter myſelf that this method will avail, except in the firſt or ſimple ſtage of the diſorder; but many caſes have a favourable ſtate for the trial in their early period, and that opportunity may be ſeized with a probability of ſucceſs.

The operation is ſimple, eaſily executed, productive of but little pain, and attended with no kind of danger.

XIV. *A Continuation of a Meteorological Diary, kept at* Fort St. George, *on the Coast of* Coromandel. *By Mr.* William Roxburgh, *Assistant-Surgeon to the Hospital at the said Fort; communicated by* Joseph Banks, *Esq. P. R. S.*

Read January 20, 1780.

	Hour from Noon.	Therm. without	Therm. within.	Barom. in Inch. &20ths.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
Mar. 1	17	67	—	30.00		0	ESE	Fair.
	½	86	80	30.00		2	ESE	Fair.
	10	78	80	30.00		2·	SE	Fair.
2	3	83	82	30.00		2·	SE	Fair.
	10	78	80	30.01		2·	SE	Fair.
3	17	75	80	30.01		·1	NW	Some clouds, no dew.
	0	85	82	30.00		2	SE	A few light clouds.
	11	79	81	30.00		·1	E	A few light clouds.
4	17	70	78	30 00		0	——	Fair.
	13	79	81	30.00		·1	E	Fair.
5	18	72	79	30.00		·1	SW	Fair, some dew.
	1	86	83	29.19		2·	S by W	A disagreeable dusty Southerly wind, which we reckon the most unhealthy wind that blows.
	11	78	80	30.00		1·	SE	Fair.
6	18	70	78	29.19		0	——	Thick fog.
	1	85	—	29.·19		2	S	A disagreeable Southerly wind.
	10	70	80	29.19		2	SSE	Fair.
7	17	70	77	29.19		0	——	Fair, much dew.
	3	83	82	29.19		2	SE	Fair.
	10	78	80	30.00		2	SE	Fair.

1777

	Hour from Noon.	Therm. without	Therm. within.	Barom. in Inch. &20ths.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	.n.			
Mar. 8	17	72	79	30.00		0	——	Fair, a good deal of dew.
	0	85	82	30.02		1	ESE	Fair.
9	17½	78	80	30.02		1·	SE	Cloudy, no dew.
	0	86	82	30.02·		1	E	Fair.
	10	78	81	30.02		1·	E	Fair.
10	18	70	77	30.02		·1	W	Fair.
	3	84	82	30.01		1·	E	Fair.
	10	78	80	30 03		1	ESE	Fair.
11	18	70	—			1	W	Fair.
	0	85	—			1	E	Fair.
	10	78	—			1	E	Fair.
12	18	70	—			0	——	Fair.
	10	78	80	30.02		1·	E	Fair.
13	17	68	76	30.02·		0	——	Fair, a little dew.
	1	85	81	30.0·1		1	ENE	Fair.
	10	79	80	30.02		·1	E	Fair.
14	18	70	78	30.01		·1	W	Fair.
	11	79	81	30.01		1	E	Fair.
15	18	72	79	30.00		·1	W	Fair.
	10	79	82	30.00		·1	N	Fair.
16	18	75	80	29.19		·1	W	Fair.
	0	87	84	30.00		2·	NNE	A few clouds.
	11	80	82	30.00		1	E	Fair, a very ſmall ſhower at 5.
17	18	78	80	29.19		1	S	Black and threatening to the Southw.
	2	80	81	29 18·	0.14	0	——	Cloudy, rained hard about 9.
	10	76	81	29.19		0	——	Cloudy, very cloſe.
18	17	72	80	29.19		0	——	Fair.
	½	85	82	29.19		1	E	Fair, feels exceedingly hot.
	11	79	81	29.19·		1·	SE	A few clouds.
19	17½	76	80	29.19		0	——	A few clouds.
	11	80	82	29.19·		1	ESE	A little haze.
20	18	73	80	29.19		0	——	Fair, a little dew.
	3	84	—	——		1·	SE	Fair.
	10	81	82	29.19		·1	ESE	Fair.
21	18	74	80	29 19		0	——	Fair.
	2	85	84	29.19		1	SE	Fair.
	10	81	83	30.00		1·	ESE	Fair.
22	18	76	81	29.19		0	——	Fair.
	½	85	84	29.·19		2	SE	Fair.
	10	81	82	29.19		1	ESE	Fair.

1777.

	Hour from Noon.	Therm. without	Therm. within.	Barom. in inch. &20ths.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
Mar. 23	18	73	81	29.19		0	——	Fair.
	10	79	82	29.19		1	SE	Fair.
24	18	72	80	29.19		1	W	Fair, a great dew.
	2	86	82	29.18		2	SSE	Fair.
Apr. 7	1	87	84	29.19		2	SE	Fair.
8	18	73	80	29.19		0	——	Fair.
	0	88	85	30.00		1·	SE	Fair.
	10	80	83	30.00		1	E	Fair.
9	18	74	80	30.00		·1	W	Fair.
	0	87	84	30.00		1·	ESE	Fair.
	10	81	81	30.00		1	E	Fair.
10	18	73	79	29.19		0	——	Fair.
	5	83	83	29.18		1·	E	Fair.
	10	80	82	29.19		1	ESE	Fair.
11	18	73	79	29.18·		0	——	Fair.
	1	87	85	29 18		2	SSE	Fair.
	10	81	82	29.18·		1·	SW	Fair.
12	18	74	80	29.·18		0	SE	Fair.
	10	81	83	29.18·		2	SSE	Fair.
13	17	79	82	29.19		1	SW	Hazy.
	1	87	85	29.18·		1	SE	Hazy.
	10	81	82	29.19		·1	SSE	Fair.
14	17	76	81	30.00		·1	W	A few clouds.
	2	87	85	29.19		2	SE	Fair.
	10	82	82	29 19·		1	S	Cloudy.
15	17	78	82	29.18		·1	N	Cloudy, beginning to rain.
	1	80	80	29.19	2.3	·1	E	Cloudy, 1.1 of rain fell the first 40′.
	11	78	79	29 19		1·	SE	Cloudy.
16	17	74	79	29.18		0	——	A few clouds.
	0	86	81	30.00		1·	ENE	Fair.
	10	79	80	29.19		·1	ENE	Fair.
17	17	73	78	29.18		·1	W	Fair.
	0	86	84	29.19		1·	E	Fair.
	11	80	81	29.18		0	——	Fair.
18	17	79	79	29.·18		0	——	Fair.
	1	87	84	29.17·		1	E	Fair.
19	17	79	82	29.18		·1	W	Fair.
	10	82	83	29 19		1	E	Fair.
20	17	79	82	29.19		0	——	Fair.
	0	86	84	29.19·		·1	SE	A few clouds; at 11 a gentle shower.

1777

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
Apr. 20	10	82	83	29.19		1·	SE	A few hazy clouds.
21	17	79	83	29.19		1	——	A few clouds.
	2	87	85	30.00		2	E	Fair.
	11	82	83	30.00		1	S	A few clouds.
22	17	77	83	30.00		0	——	Fair.
	10	82	83	29.19		1	ESE	Hazy.
23	17	77	83	30.00		0	——	Fair.
	2	87	86	29.19		2	——	Fair.
	10	83	83	29.19		2	——	Fair.
24	17	76	81	29.19		1	W	Fair.
	10	82	83	29.19		2	SE	Fair.
25	17	76	82	29.·19		0	——	Fair.
	1	87	85	29.19		2·	SE	Fair.
	10	83	84	29.19		1·	SE	Cloudy, black clouds N. with much lightning.
26	17	77	81	29.18		1	——	A few clouds.
	1	88	85	29.·18		2	SE	Cloudy.
	10	82	83	29.18		2	S	A few clouds.
27	17	76	82	29.·18		0	——	Fair.
	2	86	86	29.18		2	SSE	Fair.
	11	81	83	29.19		1	SSE	Fair.
28	17	77	82	29.18		0	——	Fair.
	1	87	85	29.18		2	SE by E	Cloudy.
	11	84	82	29.18		1	SW	Fair.
29	17	79	82	29.·18		0	——	Fair.
	1	88	87	29.18		2	SE	The S.E. half of the hemifphere clear; the N.W. half black and threatening, with thunder and lightning.
	10	80	82	29.18		0	——	Cloudy.
May 1	1	94	89	29.18		1·	WSW	Fair.
	11	84	85	29.18		2	S	Fair.
2	17	83	84	29.18		0	——	Cloudy.
	1	85	83	29.17	1.15	1	WSW	Cloudy, about forty minutes of exceeding hard rain, with almoft inceffant thunder.
	10	83	83	29.17		1	SSE	Fair, a pleafant night.
3	18	81	82	29.17·		1	SW	Fair.
	1	87	85	29.17·		2	SE	Fair.
	10	84	84	29.18		1	S	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
May 4	17	80	82	29.17		1·	S	Fair.
	1	89	85	29.·18		2	SSE	Fair.
	11	84	85	29.18·		·1	S	Dark threatening clouds to the Northward.
5	17	82	84	29 18		1·	S	Fair.
	3	87	86	29.17		2·	SE	Hazy.
	10	84	85	29.18		1·	S	Hazy.
6	17	80	82	29.18		1·	SW	Fair.
	11	83	85	29.19		3	SSE	Fair.
7	17½	81	83	29.19		1	SW	Fair.
	2	88	86	29 18		2·	SE	Fair.
	11	83	85	29.19		2	S	Fair.
8	18	80	83	29.18		1	SSW	Fair.
	11	84	84	29.19		2	S	Fair.
9	17	80	83	29.19		1	SSW	Fair.
	2	89	85	29.19		1·	ESE	Fair.
	10	85	85	29.19		1	ESE	Fair.
10	17	80	84	29.·19		·1	SW	Fair.
	2	90	86	29.·19		1·	ESE	Fair.
11	18	83	84	29.19		1	WSW	Fair.
	2	89	86	29.18		2	E	Fair.
	10	85	85	29.18		2	E	Fair.
12	17	82	84	29 18		0	——	Fair.
	0	89	86	29.19		2	ESE	Fair.
13	18	76	81	29.19	1.15	0	——	A dark, rainy morning, has rained moſt of the night.
	0	78	78	30.00		1	S	Cloudy, ſmall rain moſt part of the forenoon.
14	18	79	80	29.·18	0.3	0	——	Fair.
	1	86	83	29.18		1	SE	A few light clouds.
	9	84	83	29.18		2	SE	Fair.
15	17½	78	81	29.18		1	SW	Fair.
	0	88	85	29.18		2	SE	Fair.
	10	85	84	29.18		2	SSE	Fair.
16	18	80	83	29.18		1·	SW	Fair.
	2	89	86	29.16·		2	SE	Fair.
	10	85	85	29.17		1	S	A few clouds.
17	17½	82	83	29.17		2	SW	Fair.
	2½	89	87	29.15		2·	SSE	Hazy.
	11	84	84	29.16		1	SSW	Hazy.

1777

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain. Gage.	Winds.		State of the Weather, &c.
						Str.	Points.	
1777		°	°	Inches.	In.			
May 18	2	89	86	29 15·		2	E	Hazy.
	11	85	85	29.15·		2	SSW	Hazy.
19	18	82	83	29.15		2	SW	Hazy.
	3	88	87	29.15		3	NE	Dark, cloudy, squally weather.
	11	80	82	29.16		1·	S	Hazy, has rained hard a few miles to the S.W.
20	17	70	80	29.16		1·	SW	A little haze.
	2	88	86	29.15		2	SE	Fair.
21	17½	80	82	29.15		1·	W	A little hazy.
	1	89	86	29.15		1	E	Fair.
	11	85	85	29.16		2	S	Fair.
22	18	80	83	29.15		1	SW	Hazy.
	0	98	89	29.16·		1	WSW	Hazy.
23	18	80	82	29.15		2	SSW	Hazy; yesterday the sea breeze came in at 1.
	0	97	88	29.15·		1·	WSW	Fair.
	11	85	85	29.16		2·	SSW	Hazy; sea breeze came ½ past noon.
24	17½	80	81	29.15		2·	SSW	Hazy.
	0	87	86	29.15		2·	SE	Hazy.
	10	84	84	29.15		1	W	Cloudy, black threatening clouds to the Northward
25	21	83	82	29.15		1	W	Cloudy.
	2	87	85	29.·15		2	SE	Cloudy.
	10	83	84	29.15		2	S	Cloudy.
26	17½	83	81	29.15		2	SW	Thick haze.
	1	102	86	29.15		1	W	Thick haze.
	0	83	84	29.15		2	S	Thick haze.
27	19½	85	83	29.·16		2·	SW	Fair.
	3	92	87	29.·16		1	ENE	Fair.
	10	83	84	29.16·		2	S	Fair.
28	18	82	82	29.15		2·	SW	Fair.
	1	98	85	29.16		1	SW	Cloudy.
	10	84	84	29.16		2	S	Fair.
29	18	81	82	29 16		2	SW	Fair.
	0	100	88	29.16		1	W	Fair.
	11	83	84	29.16		2	S	Fair.
30	18	80	82	29.16		2	SW	Fair.
	1	88	86	29.16·		2	SE	Fair.
	11	82	81	29.16·		2	S	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777				Inches.	In.			
May 31	18	81°	83°	29.16		1·	SW	Fair.
	1	86	85	29.16		2	SE	Fair.
June 1	19	82	84	29.16·		1	SW	Fair.
	11	83	84	29.17		1·	S	Fair.
2	20	83	84	29.17		2·	SSW	Fair.
	2	88	85	29.·16		2	SE	Fair.
	10	84	84	29.16·		·2	S	Fair.
3	17½	81	84	29.16		1	SW	Fair.
	0	88	85	29.16·		1·	SE	Fair.
4	17½	83	84	29.·16		1	S	Fair.
	10	84	83	29.16		1·	S	Fair, much lightning to to the Weſtward.
5	18	80	83	29.16		1	SW	Fair.
	2½	98	87	29.·16		1·	SE	Fair.
	10	85	85	29.17		1·	S	Fair, a few clouds to the Southward.
6	18	82	84	29.17		1·	SW	Fair.
	0	88	85	29.17		2	SE	Fair.
	10	85	85	29.17		2	SSW	Fair.
7	10	84	85	29.17		2	SSE	Fair.
8	20	83	84	29.16		1	SW	Fair.
	2	88	86	29.15		2	SE	Fair.
	10	85	85	29.15		1	S	Fair.
9	18	·84	84	29.15		1	W	Fair.
	1	88	—	29.15		1	E	Fair.
10	18	84	84	29.15		·1	SW	Fair.
	3	90	83	29.·15		2	SE	Fair.
11	18	84	84	29.15		1	W	Hazy.
	10	84	85	29.14·		1	S	Thick haze.
12	18	85	85	29·14		2	W	Thick haze.
	3	93	87	29.14		1	NW	Cloudy.
	11	85	85	29.14		2	S	Cloudy.
13	2	96	88	29.14		1	W	Cloudy.
	10	81	84	29.·16		1	N	Cloudy, has rained a few miles to the Northward.
14	18	81	81	29 15		2	W	Cloudy.
	3	93	85	29.15		2·	W	Cloudy.
	10	86	85	29·16		1·	W	Cloudy.
15	18	82	83	29 16		2	W	Cloudy.
	10	85	84	29.15		2	W	Cloudy.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		o	o	Inches.	In.			
June 16	18	80	82	29.15		1·	W	Cloudy.
	4	92	86	29.14		2·	W	Cloudy.
	11	87	85	29.·14		2	W	Cloudy.
17	18	80	82	29.14		2	W	Cloudy.
	2	96	87	29.14·		2	W	Cloudy.
	10	86	86	29.·14		1	SW	Cloudy.
18	18	82	84	29.16·		1	W	Cloudy.
	2	98	89	29.16		2	W	Cloudy.
19	18	81	84	29.·18		.1	W	Cloudy, had a sea-breeze last night.
	10	83	85	29.17		2	S	Cloudy.
20	18	82	84	29.17		·1	W	Cloudy.
	1	97	89	29.17		1·	W	Hazy.
	11	84	87	29.·17		0	——	Hazy.
21	3	98	90	29.15		2	WSW	Cloudy, had a gentle shower in the night.
	11	88	88	29.17		1	SW	Cloudy.
22	18	80	83	29.·17		1	SW	Cloudy.
	3	95	89	29.16		2	SW	Cloudy.
	11	83	85	29.17		1	S	A little hazy.
23	3	92	89	29.16		2·	SE	Hazy, sea-breeze just come in.
	11	84	86	29.17		2	S	A little hazy.
24	18	83	84	29.18		1	SW	A little hazy.
	3	88	88	29.16		2·	SE	Fair.
25	2	100	92	29.16		1·	W	Hazy.
	11	84	86	29.17		0	——	Cloudy.
26	18	83	84	29.17		1	W	Hazy.
	3	98	92	29.16		1	W	Hazy.
	10	82	86	29.16		2	S	Cloudy.
27	½	96	88	29.16		1	W	Cloudy.
	10	87	88	29.17		1	E	Cloudy.
28	18	81	84	29.17		1	W	Cloudy.
	3	93	90	29.16		1	E	Cloudy.
	10	82	84	29.18		2	S	Cloudy.
29	18	80	83	29.17		1	W	Cloudy.
	3	89	88	29.15		1·	E	Fair.
	10	83	83	29.17		2	S	Cloudy.
30	18	79	82	29.16		1	W	Cloudy.
July 3	10	82	83	29.16		2	SW	Cloudy.

1777

	Hour from Noon.	Therm. without.	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Points	State of the Weather, &c.
1777		°	°	Inches.	In.			
July 4	18	78	81	29.16		0	SW	Cloudy.
	3	86	83	29.17		1	SE	Cloudy.
	10	82	83	29.18		1	S	Cloudy.
5	18	77	79	29.17		1	W	Cloudy.
	2	88	83	29.16		2	SE	Fair.
	10	83	84	29.18		2	S	Fair.
6	2	88	83	29.16		2	SE	Fair.
	10	83	84	29.18		2	S	Fair.
7	17	78	80	29.18		0	——	Fair.
	3	88	83	29.18		0	——	Cloudy, has rained hard to the Weftward.
	10	80	82	29.18		1	W	Fair.
8	17	77	78	29.18		2	SW	Cloudy.
10	6	86	83	29.16		2	SE	Cloudy.
	12	80	83	29.16		1	W	Cloudy.
11	18	80	82	29.16		1	W	Cloudy.
	2	92	86	29.17		1	NW	Cloudy.
	11	81	83	29.18		2	S	Cloudy.
12	18	79	80	29.17		1	SW	Cloudy.
	10	75	79	29.18		1	SW	Cloudy and hard rain,
13	18	75	78	29.17	1.0	1	W	Cloudy.
	10	78	79	29.18		1	W	Cloudy, rained laft night.
14	17	77	78	29.17	0.8	1	W	Cloudy.
	2	92	83	29.16		2	W	Fair.
	11	81	82	29 18		1	W	Cloudy.
15	19	79	79	29.16		1	W	Cloudy.
	3	95	86	29.16		2	W	Cloudy.
17	18	82	83	29.15		2	W	Cloudy.
	1	94	86	29.16		2	W	Cloudy.
18	18	81	83	29.15		1	W	Cloudy.
	3	92	85	29.16		2	W	Cloudy.
	11	77	79	29.16		0	——	Cloudy, has rained hard.
19	18	78	79	29.15	1.0	1	W	Cloudy.
20	18	80	80	29 15		2	W	Cloudy.
	9	79	83	29.16	0 10	1	SW	Cloudy.
21	4	96	86	29.15		2	W	Cloudy.
	10	84	84	29.16		1	S	Cloudy.
22	18	80	82	29.16		1	W	Cloudy.
	1	97	85	29.16		2	W	Cloudy.
	11	85	85	29.17		1	S	Cloudy.

1777

	Hour from Noon.	Therm. without	Therm. within.	Barom	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches	In.			
July 23	17	80	82	29.17		1	SW	Cloudy.
	10	81	83	29.17		3	S	Cloudy.
24	18	78	80	29.18	0.7	1	W	Cloudy.
	10	80	82	29.18		2	S	Cloudy.
25	17	78	80	29.18	0.8	1	W	Cloudy.
	3	97	86	29.17		2	W	Cloudy.
	10	80	82	29.18		2	SE	Cloudy.
26	17	78	80	29.17		1	W	Cloudy.
	1	94	84	29.18		2	W	Cloudy.
	4	86	84	29.17		2	SE	Cloudy.
	9	79	81	29.19		3	S	Cloudy.
27	18	78	80	29.17		1	W	Cloudy.
	2	95	86	29.18		2	W	Hazy.
	10	82	83	29.19		2	S	Fair.
28	18	80	80	29.19		1	W	Hazy.
	2	98	86	29.18		2	W	Hazy.
29	18	80	81	29.18		1	W	Hazy.
	1	99	86	29.19		2	W	Hazy.
	10	83	83	29.18		2	W	Hazy.
30	18	80	82	29.18		2	W	Hazy.
	2	99	90	29.18		2	W	Hazy.
	11	80	84	29.19		2	S	Hazy.
31	17	79	80	29.18	0.5	1	W	Cloudy.
	2	98	88	29.18		2	W	Cloudy.
	10	80	83	29.18		2	S	Fair.
Aug. 1	18	78	81	29.18	0.3	1	W	Cloudy.
	0	98	86	29.18		2	W	Hazy.
	11	79	82	29.19		1	SW	Hazy.
2	17	76	81	29.18	1.0	1	W	Cloudy.
	1	95	87	29.18		2	W	Fair.
	10	76	82	30.00		2	SW	Raining hard.
3	18	75	81	29.19	1.12	1	W	Hazy.
	1	90	86	29.18		1	W	Hazy.
	10	81	82	29.18		2	S	Hazy.
4	18	78	81	29.17		1	W	Hazy.
	0	89	84	29.18		2	W	Fair.
6	18	75	79	29.18	0.8	1	W	Fair.
Sept. 15	3	86	82	29.16		2	NW	Black, and threatening much rain.
16	20	80	77	29.19	1.0	0	——	Cloudy, with ſmall rain.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
Sept. 24	8	82	82	29.18		2	S	Fair.
27	18	79	82	29.18		0	——	Fair.
	0	91	84	29.19		1	SW	Hazy.
	9	83	84	29.19		0	——	Hazy.
28	18	76	80	29.18	2.4	1	W	Cloudy, much thunder, lightning, and hard rain in the night.
	1	84	82	29.19		1	NW	Cloudy.
	9	78	79	29.18		0	——	Cloudy.
29	18	77	79	29.17	0.10	0	——	Cloudy.
	9	80	80	29.18		1	SW	Cloudy, lightning.
30	18	76	79	29.17		1	W	Cloudy.
	10	82	81	29.18		1	S	Fair.
Oct. 1	18	77	78	29.18		1	W	Fair.
	5	88	84	29.19		1	E	Fair.
	10	83	82	30.00		1	SE	Fair.
2	18	78	80	30.00		1	W	Fair.
	0	88	83	30.01		1	E	Fair.
3	18	79	81	30.00		0	——	Fair.
	1	88	84	30.00		1	E	A few clouds.
	10	82	83	30.00		1	NE	Fair.
4	18	78	80	30.00		0	——	Raining.
	0	84	83	30.00		1	NE	Fair.
	11	82	83	30.00		1	NE	Fair.
5	18	77	80	29.19	0.6	1	N	Cloudy, a great dew every night.
	0	85	82	29.19		1	NE	Fair.
	9	81	82	29.18		1	NE	Fair.
6	18	77	80	29.18		1	NW	Fair.
	0	87	83	29.18		1	NE	Fair.
	9	82	82	29.19		0	——	Fair.
7	18	78	79	29.17		0	——	Cloudy.
	1	87	84	29.17		1	NE	Cloudy.
	10	82	83	29.18		0	——	Cloudy.
8	18	79	79	29.16		1	W	Cloudy.
	2	82	83	29.16		0	——	Small rain.
	10	76	78	29.16		1	W	Small rain.
9	18	77	78	29.16	0.5	0	——	A few clouds.
	0	88	83	29.16		1	SE	Fair.
10	5	84	83	29.16	0.5	2	SE	Cloudy, rained in the night.

1777

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
Oct. 11	18	80	81	29.18		1	W	Cloudy.
	0	85	83	29.19		2	SE	Fair.
	9	82	82	29.19		2	SSE	Fair.
12	18	81	81	29.19		0	——	A few clouds.
	2	87	84	29.19		2	SE	Fair.
13	18	78	81	30.00		·1	W	Fair.
	1	86	84	30.01		1	E	Cloudy.
	9	83	82	30.01		1	E	Cloudy.
14	18	78	80	30.00		·1	W	Cloudy.
	4	86	83	29.19		1	E	Fair.
15	18	78	80	29.19		0	——	Cloudy.
	11	81	82	30.01		1	E	Cloudy.
16	18	77	79	30.00	1.3	1	——	Fair, a hard shower in the night.
	3	85	83	30.00		2	NE	Fair.
	11	80	82	30.01		1	NE	Cloudy, with flying showers.
17	18	78	80	30.00	1.0	0	——	Cloudy, hard squalls in the night.
	10	81	81	30.01		1	E	Cloudy, showers through the day.
18	18	77	80	30.00	1.16	0	——	Raining.
	7	82	80	29.19		1	E	Fair.
19	17½	78	80	30.00	0.4	0	——	Cloudy, thund. and lightn.
20	18	78	79	30.02		0	——	Fair.
21	3	86	84	29.18		2	NE	Fair.
22	18	78	80	29.18		0	——	A few clouds.
	1	79	80	29.18		·1	N	Cloudy, small rain.
	11	77	79	29.18		·1	N	Raining.
23	18	76	78	29.18	0.12	1	W	Cloudy.
	9	80	81	29.18		1	S	Cloudy.
24	18	78	78	29.17		1	S	Fair.
	10	83	82	29.18		0	——	A few clouds.
25	18	80	80	29.18		1	SW	Fair.
26	10	83	83	29.19		2	E	Fair.
27	18	80	80	29.19		0	——	Fair.
	0	85	84	30.00		1	E	Fair.
28	18	79	80	30.00		1	NW	Fair, NAIRNE's Marine bar. 29°⅛.
29	9	81	80	30.01		2	E	Fair.
30	18	76	78	30.00	0.6	·1	NW	Cloudy.
	1	85	81	30.00		1	NE	Fair.
	10	80	81	30.00		2	N	Cloudy.
31	18	79	80	30.00		·1	NW	Cloudy.
	3	85	82	30.00		·1	E	Cloudy.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Points.	State of the Weather, &c.
1777		°	°	Inches.	In.			
Nov. 1	18	74	78	30.00		0	——	Cloudy.
	3	82	80	30.00		2	N	Cloudy.
	9	76	79	30.00		2	N	Cloudy.
3	18	70	75	30.00		1·	N	Fair.
	2	85	81	30.00		·2	NNE	Fair.
	10	76	79	30.00		1·	N	Fair.
4	18	68	75	30.00		·1	NW	Fair, much dew.
	2	85	81	30.00		1·	NNE	Hazy.
5	18	72	75	30.00		1	NW	Fair.
	2	85	80	30.00		3	N	Fair.
6	18	75	79	30.00	0.2	0	——	Cloudy.
	2	76	78	29.19		1·	N	Cloudy, a little ſmall rain.
	10	78	78	30.00		1	N	Cloudy.
7	2	84	80	29.19		1	NE	Cloudy.
	10	77	79	30.00		0	——	Hazy.
8	18	75	77	30.00		1	NW	Cloudy.
	2	85	81	29.19		1·	E	Fair.
9	18	76	79	30.00		1	NW	Cloudy, a little ſmall rain.
	1	83	80	29.19		2	NNE	Light flying ſhowers.
10	18	78	79	29.19	0.3	1	N	Cloudy.
	0	85	83	30.00		2	NNE	Fair.
11	18	76	79	30.00		1	NW	Fair.
	10	79	81	30.00		1	NE	Hazy.
12	18	76	79	30.00		1	NW	Hazy.
	2	85	82	29.19		3	NNW	Hazy.
13	20	75	76	30.00		2	N	Cloudy.
	8	77	79	30.00		1	NNW	Cloudy.
14	17	70	74	29.18		1	NW	Cloudy.
	2	71	74	29.17		1	N	Dark clouds, with thunder and lightning.
	10	69	73	29.18		2	N	Dark clouds, with thunder and lightning, with rain.
15	19	65	72	29.17	1.9	2	WNW	Cloudy, raining.
	0	72	74	29.17		3	N	Cloudy.
	10	70	73	29.18		2	NW	Cloudy, frequent ſhowers.
16	19	72	72	29.18	1.0	2	NW	Cloudy, frequent ſhowers.
	1	81	78	29.19		2	N	Fair.
	9	78	78	29.19		2	NE	Cloudy.
17	20	78	77	29.19		1	NW	Cloudy.
	2	80	78	29.19		2	NE	Cloudy, rained in the forenoon.

1777

	Hours from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches	In.			
Nov. 18	19	72	75	29.19	0.14	1	NW	Fair.
	2	81	78	29.19		2	NE	Fair.
	10	76	76	30.00		1	N	Fair.
19	18	72	73	30.00		1	NW	Fair, ſome dew on the graſs.
20	18	67	73	30.01		1	NW	Fair, a great dew.
	1	81	78	30.01		2	N	Fair.
	10	73	75	30.02		.1	N	Fair.
21	18	66	72	30.02		·1	NW	Fair, a great dew.
	0	81	77	30.03		2	NNE	Fair.
	11	75	75	30.0·4		2	NNE	Fair, NAIRNE's marine bar. $29^{\circ}\frac{8}{10}$.
22	18	70	73	30.03		1	W	Fair, low black clouds to Weſtward.
	3	81	78	30.02		2	NE	Fair.
	10	75	75	30.03		3	N	Fair.
23	18	71	75	30.02		1	NW	Fair.
	0	81	78	30.02		3	N	Fair.
	10	77	77	30.02·		2	N	Fair.
24	18	73	76	30.01		1	N	Fair.
25	18	70	74	30.00	2.10	2	N	Much rain and wind through the night.
	10	77	77	30.0·1		1	N	Cloudy.
26	18	75	75	29.19	1.0	2·	NNE	Cloudy.
	0	77	76	30.01		2	NNE	Cloudy.
	10	77	75	30.01		2·	N	Cloudy.
27	18	74	75	30.00		1	N	Cloudy.
	3	80	76	30.00		2·	NNE	Fair.
	10	76	75	30.00		1·	N	Fair.
28	18	73	74	30.00		1	NW	Cloudy.
	10	75	76	30.01		2	N	Fair.
29	18	70	74	30.00		1	NW	Fair.
	10	75	75	30.02		1·	N	Fair.
30	8	69	74	30.02		1	NW	Fair, dew on the graſs.
	1	83	79	30.02		2·	NE	Fair.
	10	76	76	30.03		2·	N	Cloudy.
Dec. 1	18	75	75	30.02		1	NW	Cloudy.
	23	82	78	30.03·		2·	NNE	Fair, NAIRNE's marine bar. $29^{\circ}\frac{8}{10}$.
	10	77	78	30.02		2	N	Fair, NAIRNE's marine bar. $29^{\circ}\frac{8}{10}$.
2	18	71	75	30.02		1	NW	Fair.
	2	82	78	30.01		2·	NNE	Cloudy.
	10	77	77	30.01		1	NNE	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777		°	°	Inches	In.			
Dec. 3	18	70	74	30.0·1		1	NW	Fair, dew.
	2	86	78	30.01		3	NE	Fair.
	11	73	75	30.02		1	N	Fair.
4	18	68	73	30.01		1	NW	Fair, dew.
	10	75	76	30.02		1	N	Fair.
5	18	69	73	30.01		1	NW	Fair, dew.
	0	82	78	30.02		2	NE	Fair.
6	18	70	75	30.01		1	NW	Fair, dew.
7	18	70	74	30.02		1	NW	Fair, dew.
	10	75	77	30.02		2	N	Fair.
8	1	82	79	30.02		3	NNE	A few clouds.
9	18	74	76	30.02		2	NNE	Fair.
	4	80	79	30.02		3	NE	Fair, no dew.
10	18	73	74	30.02		2	NE	Cloudy.
	0	83	77	30.03		3	NE	Fair.
11	18	70	—	30.02	0·2	1	NNW	Cloudy.
12	18	69	72	30.00		2	N	Raining.
	0	71	74	30.01		2	NNE	Rain juſt over.
	10	75	76	30.00		2	N	Frequent ſhowers.
13	18	74	76	29.19	2·2	3	NE	Raining.
	0	80	76	30.01		2	NE	A few clouds.
	10	74	76	30.02		2	N	A few clouds.
14	18	74	75	30.01	0·19	2	NNE	A few clouds.
	1	81	78	30.01·		3	NNE	A few clouds, NAIRNE's mar.b.29° $7\frac{1}{10}$
	10	76	76	30.02		2	N	A few clouds.
15	12	70	74	30.02		1	N	Fair.
16	18	68	74	30.02		0	——	Fair, much dew.
	12	70	74	30.02		1	N	Fair.
19	23	81	78	30.04·		2	NE	Fair.
20	18	74	75	30.02·		1	N	Fair.
	23	80	78	30.03·		2	NNE	Fair.
	11	76	77	30.03		2	NNE	Fair.
21	18	74	75	30.02		1	NNE	Cloudy.
	0	81	77	30.04·		3	NNE	Cloudy.
	10	74	76	30·04·		1	N	Cloudy.
22	18	73	—	30.03		1	N	Cloudy.
	10	73	75	30.03		1	N	Fair.
23	18	66	—	30.04		1	NW	Fair, ſome dew.
	11	75	75	30.03		3	NNE	A few clouds.

1777

	Hour from Noon.	Therm. without	Therm. within	Barom.	Rain. Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1777				Inches.	In.			
Dec. 24	18	73	—	30.02		1	NW	Cloudy.
	10	76	76	30.01		2	N	A few clouds.
25	18	76	76	30.0·1	0·4	2	NE	Cloudy and threatens rain.
	10	76	76	30.0·2		2	N	Fair.
26	4	72	76	30.01		1	N	Small rain.
	10	72	75	30.01		1	N	Cloudy.
27	0	80	77	30.01		2	N	Cloudy.
28	18	73	74	30.00		1	N	Fair.
	23½	81	78	30.01		3	NNE	Cloudy.
	9	75	75	30.00		2	N	Fair.
29	18	67	74	30.00		1	NW	Fair, dew.
	2	79	77	30.01		2	NE	
30	18	69	—	30.01		1	NNW	Cloudy.
	11	72	75	30.01		1	N	A few clouds.
31	18	71	74	30.01		1	NW	A few clouds.
1778								
Jan. 1	18	67	74	30.01		1	NW	Fair.
2	18	69	73	30.01		1	NW	Fair.
	1	81	76	30.02		2	NE	Fair.
3	8	75	76	30.02		1	NE	Fair.
4	18	74	—	30.02		1	NE	Cloudy.
5	18	68	74	30.01		1	NW	Cloudy.
	1	80	77	30.02		3	NE	Cloudy.
	10	74	75	30.02		1	N	Fair.
7	0	79	76	30.01		2	NE	Fair.
	10	72	75	30.02		1	N	Fair.
8	18	68	74	30.02		1	NW	Fair.
9	18	67	73	30.01		1	NW	Hazy.
	0	78	76	30.01		1	NE	Hazy.
	10	73	74	30.01		2	NE	Fair.
10	18	69	74	30.01		1	N	Fair.
13	18	66	71	30.00		1	NW	Fair, a great dew.
14	18	64	70	30.00		0	—	Fair, a great dew.
	23	77	75	30.01		1	NE	Fair.
15	19	65	70	30.00		0	—	Fair, a great dew.
	11	74	74	30.01		1	N	Fair.
16	18	66	71	30.0·1		1	NW	A few clouds.
	1	79	76	30.0·1		1	NE	Fair.
17	18	65	71	30.00		1	NW	A little foggy.
	10	75	75	30.01		1	NE	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1778		°	°	inches.	In.			
Jan. 18	18	69	73	30.02		1	NW	Fair, a little dew.
	23	80	77	30.03		·1	ESE	Fair.
	10	76	76	30.02		1	ESE	Fair.
19	18	67	74	30.02		1	NW	Fair.
	9	73	76	30.03		1	NE	Fair.
20	18	69	74	30.02		1	N	Fair.
	10	75	75	30.0·4		1·	N	Fair.
21	18	68	73	30.03		1	NW	Fair.
	2	80	78	30.03		2	NE	Fair.
22	18	67	71	30.02		0	——	Fair.
	2	80	77	30.02		2	ENE	Fair.
23	18	67	72	30.02		1	NW	Fair.
	10	74	75	30.02		1	ENE	Fair.
24	18	66	71	30.00		1	NW	Fair.
	1	82	77	30.01		1	NE	Hazy.
25	18	68	72	30.0·1		·1	NW	Fair.
31	18	69	74	30.01		·1	NW	Fair.
	½	82	78	30.02		2·	ENE	Fair.
Feb. 2	18	69	73	30.02		1	NW	Fair.
	4	80	78	30.02		3	NNE	Fair.
3	18	76	76	30.02		2	NNE	Cloudy.
5	18	65	71	30.00		·1	NW	Fair, dew.
	10	74	77	30.0·1		1	ENE	Fair.
6	18	65	70	30.00		0	——	Fair, dew.
7	21	79	76	30.02·		·1	SE	Fair.
	10	76	77	30.01·		·1	SE	Fair.
8	19	66	71	30.0·1		0	——	Fair.
	2	80	76	30.0·2		1·	ESE	Fair.
	10	75	75	30.02·		1	E	Fair.
9	18	68	73	30.0·2		1	W	Fair.
10	18	68	72	30.02		1	W	Fair.
11	2	82	79	30.02		1·	NNE	Fair.
	10	75	78	30.02		1·	NNE	Fair.
12	18	71	75	30.0·2		1·	NW	Fair.
	3	81	78	30.01·		2·	NNE	Fair.
	10	75	77	30.01		1	NE	Fair.
13	18	67	72	30.01		1	NW	Fair.
	10	75	77	30.02		0	——	Fair.

1778

	Hour from Noon.	Therm. without	Therm. within.	Barom	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1778		°	°	Inches	In.			
Feb. 14	18	69	74	30.01		1	NW	Fair.
	2	81	78	30.01		·2	NE	Fair.
	11	74	76	30.02		·1	NE	Fair.
15	18	67	72	30.01·		1	NW	Fair.
	3	81	79	30.01		2	NE	Fair.
	10	76	76	30.01·		2	N	Fair.
16	18	70	75	30.01		1	NW	Cloudy.
	10	74	76	30.02		0	——	Fair.
17	18	67	71	30.01		1	NW	Fair.
	4	81	78	30.01		2	E	Fair.
18	18	70	73	30.02		1	W	Cloudy.
	3	81	79	30.02		1·	E	Fair.
	10	79	76	30.02·		1·	E	Fair.
19	18	77	78	30.02·		2	E	Fair.
	9	79	79	30.0·4		2	NE	Cloudy.
20	0	84	80	30.0·3		2	ESE	Fair.
22	18	72	78	30.02		0	——	Hazy.
23	18	73	78	30.02		0	——	Fair.
24	18	71	70	30.01		0	——	Fair.
	23	84	80	30.03		1	NE	Fair.
	9	77	78	30.02		1	NE	Fair.
25	18	69	75	30.01·		·1	W	Fair, much dew.
	2	86	79	30.0·1		1	E	Fair.
26	18	68	74	30.01		1	W	Fair.
27	18	68	74	30.01		0	——	Fair.
	0	81	78	30.0·1		2	SE	Fair.
	11	77	78	30.01		1	SE	Fair.
Mar. 1	3	82	79	30.00		3	SE	Fair.
	11	78	78	30.01		2	SSE	Fair.
2	18	77	76	30.00		·1	W	Fair.
	10	80	80	30.00		2	S	Fair.
4	10	80	80	30.00		2	SE	Fair.
5	18	74	78	30.00		0	——	Fair.
	10	78	78	30.00		2	E	Fair.
6	18	72	75	30.00		·1	W	Fair.
	0	83	80	30.00		1	E	Fair.
8	10	77	78	30.02		1	E	Fair.
9	18	70	74	30.02·		·1	W	Fair.
	0	81	79	30.02		1·	E	Fair.

	Hours from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1778		°	°	Inches.	In.			
Mar. 10	18	70	75	30.01		0	——	Fair.
	11	79	78	30.01		1	E	Fair.
11	18	69	75	30.01		1	W	Fair.
	3	83	80	30.00		2	E	Fair.
	10	80	79	30.00		2	ESE	Fair.
12	10	79	79	30.00		2	ESE	Fair.
14	18	76	79	30.00		0	——	Fair.
17	1	84	81	30.01		2	SE	Fair.
	10	80	80	30.01		1	E	Fair.
18	18	73	78	30.01		0	——	Fair.
	3	85	81	29.19		2	SE	Fair.
	10	80	80	30.00		2	SE	Fair.
19	18	74	78	29.19		1	W	Fair.
	0	84	80	30.00		1	E	Fair.
	10	80	80	29.19		2	S	Fair.
20	18	73	78	29.19		1	W	Fair.
	2	85	81	29.19		3	SE	Fair.
21	18	76	78	29.19		1	W	Fair.
23	10	79	81	29.18		2	SE	Fair.
24	2	87	83	29.17		3	S	Fair.
	10	81	81	29.18		2	S	Fair.
25	18	80	81	29.19		1	SW	Fair.
	0	89	84	29.19		3	S	Fair.
	10	81	81	29.18		2	S	Fair.
26	18	79	80	29.19		2	S	Fair.
	3	88	84	29.18		2	S	Fair.
27	18	78	80	29.19		2	S	Fair.
28	19	81	82	30.01		1	SE	Fair.
29	18	76	79	30.01		0	——	Fair.
	0	86	83	30.01		2	SE	Fair.
	10	81	80	30.01		1	SE	Fair.
30	18	78	80	30.01		0	——	Fair.
	10	81	82	30.01		2	SE	Fair.
31	18	76	80	30.01		0	——	Fair.
	3	86	82	30.01		1	E	Fair.
	10	80	80	30.01		1	SE	Fair.
Apr. 1	18	74	78	30.00		0	——	Fair.
	2	84	82	30.00		2	E	Fair.
2	18	74	77	30.00		0	——	A few light clouds.

1778

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points.	State of the Weather, &c.
1778		°	°	Inches.	In.			
Apr. 3	18	73	77	30.00		0	——	Fair.
	6	83	82	30.00		2	SE	Fair.
4	23	78	81	30.02		2	E	Fair.
6	21	86	81	30.02		1	S	Fair.
9	23	86	82	30.01		2	SE	Fair.
10	17½	73	77	30.00		0	——	Fair, a great dew.
	2	88	83	29.19		3	S	Fair.
11	20	85	82	30.00		3	S	Fair, a great dew.
	23	94	84	30.01		1	SW	Fair, very hot.
12	8	83	83	30.00		1·	SSE	Fair.
14	18	81	79	30.00		1	W	Fair.
16	18	80	79	29.18		2	S	Fair, a ſtrong ſoutherly wind all night.
	10	82	82	29.19		2	SE	Fair.
17	0	89	84	30.00		3	S	Fair.
18	12	81	82	30.02		1	S	Fair.
19	17½	76	80	30.02		0	——	Foggy.
20	20	85	81	30.01		1	SW	Fair.
21	21½	85	81	30.00		2	SSE	Fair.
22	21	86	82	30.00		1	S	Fair.
	5	86	84	29.19		1	E	Fair.
23	17	82	81	30.00		1	E	Fair.
	23	88	83	30.01		1	NE	Fair, had a very gentle ſhower at 9.
24	17	82	81	30.00		1·	E	Cloudy round the horizon.
	0	86	83	30.00		3	SE	Cloudy round the horizon.
25	17	78	80	30.00		0		Hazy.
	7	84	83	29.19		2	SE	Clouds to the Weſtward, with much lightning.
26	22	89	84	30.00		2	SSE	Fair.
	11	83	83	29.18		2	S	Fair, lightning to the Weſtward.
27	17	81	82	29.17		1·	SW	Fair.
28	17	82	83	29.16		2	S	Cloudy.
29	21	87	84	29.19		2 ·1	S	Fair, has been little dew theſe ten days paſt.
30	19	82	83	29.19			S	Fair.
	8	82	83	29.19		1·	SE	Fair.
May 1	17½	77	80	29.19		0	——	Fair.
2	21½	90	84	29.19		2·	SSW	Fair.
	6	85	84	29.19		1·	SE	Fair.
3	17	80	84	29.18		1	SSW	Fair.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds. Str.	Winds. Points	State of the Weather, &c.
1778.		°	°	Inches.	In.			
May 3	7	84	84	29.17		1	S	Cloudy, has been ſqually, with thunder and lightning to the Weſtward.
5	17½	82	82	29.18		1	S	Fair.
	23	89	84	29.19		2	SE	Dark black clouds to the N.W.
7	17	80	80	29.17		·1	SW	Fair.
8	20	87	84	29.18		·1	SW	Fair.
	4	89	85	29.16		1·	SSE	Fair, clouds to the Weſtw.
	11	83	85	29.18		·1	S	Cloudy.
9	17½	80	81	29.17		0		Cloudy.
10	22	89	84	29.19		1	SE	Fair.
	10	84	84	29.17		1	SE	Fair.
11	0	82	83	29.17		1	S	Fair.
12	1	88	85	29.18		2	SE	Fair.
13	1	88	85	29.18		2	SE	Fair.
	10	83	84	29.18		2·	S	Fair.
14	17½	82	83	29.18		1	SW	Fair.
	2	88	85	29.18		2	SSE	Fair.
15	22	88	85	29.19		1	SE	Fair.
16	19	85	83	29.18		0		Fair.
17	18	81	83	29.18		0	W	Fair.
	23	100	89	29.18		1	W	Fair, firſt day of the land wind.
	6	88	86	29.16		2	SW	Cloudy, flying ſhowers to the Weſtw.
18	17½	82	83	29.17		1·	W	Fair.
	0	104	90	29.18		1	W	Fair, exceeding hot.
	1	90	89	29.18		2	SE	Cloudy, ſea breeze came in half an hour ago.
19	20	90	84	29.18		1·	SW	Cloudy.
	0	100	89	29.19		·1	W	Cloudy, ſea breeze in ſight.
	¼	90	88	29.18		2·	SE	Cloudy, a freſh ſea breeze.
	7	84	84	29.17		1	S	Cloudy, thunder in the afternoon, much lightning.
20	17½	80	83	29.18		1	W	Fair.
	0	93	88	29.19		1	E	Fair, ſea breeze juſt come in.
	10½	84	83	29.18		3	S	Fair.
21	21	93	86	29.18		·1	S	Fair.
	6	85	85	29.16		2·	SSE	Fair, a ſtrong S.W. wind all day.
22	19	82	83	29.17		0		Fair.
	0	100	89	29.18		0		Fair, ſea breeze coming in.

	Hour from Noon.	Therm. without	Therm. within.	Barom.	Rain Gage.	Winds.		State of the Weather, &c.
						Str.	Points.	
1778		°	°	Inches.	In.			
May 23	20	90	85	29.18		2	W	Fair.
	3	88	85	29.16		1	E	Cloudy.
	12	84	85	29.18		0		Cloudy.
25	0	99	89	29.17		2	S	Fair.
	12	84	84	29.16		·1	W	Cloudy.
26	0	104	90	29.16		·1	W	Hazy.
27	18	81	82	29.15		1·	W	Cloudy.
	1	101	90	29.16		0		Cloudy.
28	18	82	83	2 .15		1	SW	Fair.
	1	101	90	29.17		0	——	Fair, thermometer in the ☉ 132°.
29	21	86	83	29.18		·1	NE	Cloudy.
	4	101	89	29.16		1	W	Fair.
30	17½	84	84	29.16		2	W	Fair.
	12	85	86	29.15		2	S	Fair, ſea-breeze came in at 1.
31	0	100	90	29.17		2	ENE	Fair, juſt come in.

A table of the greateſt, leaſt, and mean heights of the thermometer [a] and barometer in each month, from March 1777 to May 1778.

Month.	Thermometer			Barometer		
	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.
	°	°	°	Inches.	Inches	Inches.
March	87	67	77	30.03	29.18	30.00½
April	88	73	80½	30.00	29.17	29.18½
May	102	76	88½	30.00	29.15	29.17½
June	100	79	89½	29.18	29.14	29.16
July	99	75	87	29.19	29.15	29.17
Auguſt	98	75	86½	30.00	29.17	29.18½
September	91	76	83½	29.19	29.16	29.17½
October	88	76	82	30.02	29.16	29.19
November	85	65	75	30.04	29.17	30.00½
December	87	66	76½	30.04	29.19	30.01½
1778 January	82	64	73	30.04	30.00	30.02
February	86	65	76½	30.04	30.00	30.02
March	89	69	79	30.02	29.17	30.00½
April	94	73	83½	30.02	29.16	29.19
May	104	77	90½	29.19	29.15	29.17

(a) The thermometer without doors is meant.

A table

A table of the number of European patients admitted into, discharged from, and who died in, Fort St. George Hospital; also an account of the different diseases, taken at the end of each month, during part of the year 1777.

	Diseases.	Mar.	Apr.	May	June	July	Aug.	Sept.	Oct.	Nov.	Dec.
1.	Continued fevers,	12	13					15	9	6	4
2.	Intermitting fevers,	2	0					1	0	1	1
3.	Hepatitis; here it is commonly called the liver,	9	14					10	7	4	3
4.	Liver cough,	8	3					4	4	1	3
5.	Liver flux,	11	8					11	11	7	8
6.	Bilious, where there are no evident obstructions and little fever,	9	12					7	6	6	6
7.	Fever and flux,	0	5					0	2	2	0
8.	Diarrhæa and dysenteria,	20	19					18	23	43	34
9.	Rheumatism, old venereal pains, &c.	24	20					28	33	31	30
10.	Abdominal obstructions, with little or no fever,	12	10					19	13	7	10
11.	Phthisis pulmonalis,	1	1					2	1	0	1
12.	Coughs and colds,	6	5					4	9	8	7
13.	Epilepsy,	1	2					3	0	1	0
14.	Nervous,	2	2					2	3	2	2
15.	Dropsy,	1	2					2	3	1	1
16.	Jaundice,	0	0					0	0	0	0
17.	Itch,	2	0					3	0	1	2
18.	Gravel,	7	5					4	5	2	1
19.	Diabetes,	1	3					1	0	0	1
20.	Strangury,	0	0					4	2	3	4
21.	Diseases of the eyes,	4	3					0	0	0	0
22.	Venereal,	48	44					37	43	37	40
23.	Piles,	3	2					2	3	1	1
24.	Fistula,	1	1					0	0	0	0
25.	Rupture,	0	1					0	0	0	0
26.	Surgical, mostly old ulcers,	17	18					22	16	15	16
27.	Anomalous,	6	4					11	8	4	4
28.	Accidents,	3	6					10	2	3	1
	Received into the hospital,	116	110	146	141	119	121	111	104	84	109
	Discharged cured,	96	121	123	147	122	93	111	103	84	74
	Deaths,	2	2	3	6	7	2	3	5	8	8
	Remains,	210	203					220	203	186	180

A table of the number of European patients admitted into, discharged from, and who died in, Fort St. George Hospital; also an account of the different diseases, taken at the end of each month, during part of the year 1778.

Diseases.	Jan.	Feb.	Mar.	Apr.	May
1. Continued fevers,	6	9	7	10	18
2. Intermitting fevers,	0	0	1	0	0
3. Hepatitis; here it is commonly called the liver,	7	6	7	8	3
4. Liver cough,	6	2	1	3	3
5. Liver flux,	6	6	6	14	7
6. { Bilious where there are no evident obstructions and little fevers,	4	4	11	7	6
7. Fever and flux,	0	0	3	3	4
8. Diarrhœa and dysenteria,	24	25	34	27	40
9. Rheumatism, old venereal pains, &c.	26	17	16	28	29
10. Abdominal obstructions, with little or no fever,	9	13	8	13	8
11. Phthisis pulmonalis,	1	0	2	1	0
12. Coughs and colds,	5	7	8	9	6
13. Epilepsy,	0	0	1	1	0
14. Nervous,	3	6	4	1	1
15. Dropsy,	1	1	0	0	0
16. Jaundice,	1	1	0	0	0
17. Itch,	2	1	1	2	1
18. Gravel,	1	1	1	0	1
19. Diabetes,	0	0	1	1	1
20. Strangury,	3	5	4	1	4
21. Diseases of the eyes,	1	0	0	3	2
22. Venereal,	43	34	37	45	31
23. Piles,	0	1	1	0	0
24. Fistula,	0	1	1	0	0
25. Rupture,	0	1	1	1	1
26. Surgical, mostly old ulcers,	18	11	7	14	13
27. Anomalous,	6	6	3	9	8
28. Accidents,	3	4	3	6	10
Received into the hospital,	125	113	63	94	85
Discharged cured,	74	77	85	119	110
Deaths,	5	5	2	3	4
Remains,	176	162	170	207	197

You

You will find the numbers remaining in the hoſpital do not agree with the number received, diſcharged, and deceaſed, becauſe it frequently happens, that patients are received and diſcharged between the beginning and end of the month, of courſe cannot come into the number remaining at the end of the month.

The number received and diſcharged are taken from the hoſpital books, kept by a clerk.

XV. *A Journal of the Weather at* Montreal. *By Mr.* Barr, *Purveyor to his Majesty's Hospitals in* Canada; *communicated by Dr.* Saunders.

Read January 20, 1780.

THE winter seemed to set in about the middle of November; from that time to the end of the month we had some snow and cold weather; but the thermometer never was lower than 16° or 18° below the freezing point.

About the 24th and 25th of December people crossed the river three miles below Montreal.

The letters a and b after the figures denote the mercury to be above or below o. The thermometer from which these observations were taken, was of FAHRENHEIT's construction; it was suspended to a post, perfectly in the shade, and no building nearer to it to reflect the heat than five or six feet.

1778

FOR DECEMBER, 1778.

Day.	Thermometer. Morning	Thermometer. Evening	Snow.	Rain.	Winds.	Remarks.
	°	°	Inch.			
1	24 a o	28 a o	4		SW	A great fall of ſnow laſt night.
2	31 a o	28 a o			W	Some ſnow this afternoon.
3	11 a o	19 a o			NW	Cloudy.
4	27 a o	30 a o			W	Cloudy with a little ſnow.
5	33 a o	33 a o		Rain	W	Cloudy.
6	29 a o	18 a o			E	Cloudy.
7	o	6 a o			E	Cloudy.
8	14 a o	8 a o	6		E	Cloudy, it ſnowed all day.
9	4 b o	2 a o			NW	Clear.
10	4 a o	16 a o			NE	Cloudy, with a little ſnow.
11	9 a o	7 a o			E	Cloudy.
12	4 b o	7 a o			NE	Clear.
13	14 a o	20 a o	12		NE	Cloudy, the wind high with a great drift.
14	16 a o	o			W	Cloudy.
15	6 b o	7 a o			SW	Cloudy.
16	1 a o	12 a o			NW	Clear.
17	32 a o	33 a o		Rain.	Calm.	Cloudy, and a thick fog and rain.
18	32 a o	25 a o			SW	Cloudy.
19	12 a o	16 a o			NE	Cloudy.
20	19 a o	32 a o		Rain.	NE	Cloudy, the wind S. in the evening.
21	16 a o	15 a o			E	Cloudy.
22	7 b o	4 b o			NW	Clear, and very little wind.
23	12 b o	6 b o			W	Clear, and very little wind.
24	14 b o	12 b o			NW	Clear.
25	21 b o	18 b o			NE	Clear.
26	8 b o	3 b o			NE	Cloudy, with ſome ſnow.
27	9 b o	1 b o			N	Clear.
28	5 b o	4 a o			W	Clear.
29	8 a o	21 a o			N	Clear.
30	31 a o	21 a o			W	Clear.
31	18 a o	32 a o			W	Clear.

FOR JANUARY, 1779.

Day.	Thermometer. Morning	Evening.	Snow.	Rain.	Winds.	Remarks.
	°	°	Inch.			
1	33 a o	29 a o			W	Clear.
2	22 a o	32 a o		Rain.	NW	Clear.
3	37 a o	14 a o			SW	Clear.
4	6 a o	14 a o			NW	Clear.
5	11 a o	20 a o			W	Clear.
6	16 a o	7 a o			W	Cloudy.
7	2 a o	7 a o			NE	Cloudy.
8	3 a o	6 a o			N	Clear.
9	4 a o	29 a o			NE	Cloudy, with a little ſnow.
10	36 a o	8 a o		Rain.	SW	Cloudy, in the morning rain.
11	12 b o	7 b o			NW	Clear.
12	5 a o	18 a o			SW	Cloudy, with ſome ſnow.
13	15 a o	20 a o			W	Clear.
14	14 a o	18 a o			W	Clear.
15	10 a o	21 a o			NW	Clear.
16	3 a o	6 a o			NW	Clear.
17	5 a o	7 a o	4		NE	Cloudy, with ſnow all day.
18	11 b o	7 b o			NW	Clear.
*19	22 b o	12 a o			NE	Cloudy, with ſome ſnow.
20	15 a o	8 a o			W	Clear.
21	4 a o	7 a o			W	Clear.
22	8 b o	8 a o	2		NW	Clear, and almoſt calm.
23	2 a o	12 a o	4		NE	Cloudy, with ſnow.
24	10 a o	16 a o	4		NE	Cloudy, with ſnow.
25	17 a o	25 a o	6		NE	Cloudy, a great fall laſt night.
26	25 a o	12 a o			N by W	Cloudy.
†27	o	6 a o			NE	Clear.
28	20 a o	22 a o			W by S	Cloudy, with ſome ſnow.
29	5 a o	11 a o			NE	Cloudy.
30	4 b o	23 a o	4		N by W	Clear, but ſnow in the evening.
31	22 a o	26 a o			W	Cloudy.

* This day 16 or 17 German ſoldiers were frozen to death in croſſing the ice on the Lake St. Pierre, a lake near Trois Rivieres; and about double the number were froſt bitten, many of whom loſt their feet and hands.

† Uncommon to have the weather clear when the wind is eaſterly.

FOR FEBRUARY, 1779.

Days.	Thermometer. Morning	Thermometer. Evening.	Snow.	Rain.	Winds.	Remarks.
	°	°	Inch.			
1	33 a 0	34 a 0			W by E	Cloudy.
2	40 a 0	45 a 0		Rain.	SW	Cloudy, with a thick fog.
3	30 a 0	26 a 0			NE	Cloudy.
4	28 a 0	27 a 0			SW	Cloudy, with a little ſnow.
5	7 a 0	10 a 0			W by N	Clear.
6	1 a 0	15 a 0			N by E	Clear.
7	14 a 0	29 a 0	4		NE	Cloudy, with ſnow.
8	5 a 0	8 a 0			NE	Clear.
9	11 a 0	23 a 0	4		NE	Cloudy, with ſnow laſt night.
10	24 a 0	35 a 0			Calm	Foggy.
11	21 a 0	16 a 0			W	Clear.
12	13 a 0	37 a 0			W by S	Clear.
13	30 a 0	31 a 0			W	Clear.
14	20 a 0	25 a 0	2		NE	Cloudy, with ſnow.
15	23 a 0	24 a 0			NE	Cloudy.
16	14 a 0	24 a 0			NE	Cloudy.
17	22 a 0	38 a 0			NE	Cloudy, and the wind high.
18	27 a 0	27 a 0			W	Clear.
19	34 a 0	36 a 0			W	Cloudy.
20	34 a 0	19 a 0			W	Cloudy.
21	9 a 0	35 a 0			N by E	Cloudy, the wind S. at night.
22	38 a 0	39 a 0			SW	Cloudy.
23	31 a 0	32 a 0		Rain.	W by N	Clear.
24	14 a 0	20 a 0			N	Clear.
25	19 a 0	22 a 0			W by N	Cloudy.
26	19 a 0	34 a 0	4		W	Cloudy, with ſnow all day.
27	26 a 0	31 a 0			E	Cloudy.
28	31 a 0	39 a 0		Rain.	NE	Cloudy, with a fog and rain.

FOR MARCH, 1779.

Days.	Thermometer. Morning	Thermometer. Evening.	Snow.	Rain.	Winds.	Remarks.
	°	°	Inch.			
1	28 a 0	28 a 0			N by E	Clear.
2	21 a 0	38 a 0		Rain.	W by S	Cloudy, with rain.
3	37 a 0	32 a 0			W by S	Clear.
4	17 a 0	24 a 0			N	Clear.
5	18 a 0	28 a 0	4		N by E	Cloudy, with ſnow all day.
6	19 a 0	23 a 0			N by E	Clear.
7	18 a 0	29 a 0			NE	Clear.
8	30 a 0	20 a 0	2		W	Cloudy, with ſome ſnow.
9	24 a 0	28 a 0			NW	Clear.
10	10 a 0	19 a 0			N	Clear.
11	10 a 0	20 a 0			N	Clear.
12	16 a 0	25 a 0	6		NE	Cloudy, with a great fall of ſnow.
13	21 a 0	22 a 0			W by N	Cloudy.
14	1 a 0	19 a 0			E	Clear and almoſt calm.
15	19 a 0	21 a 0			E	Clear.
16	11 a 0	23 a 0			NW	Clear.
17	11 a 0	14 a 0			NW	Clear.
18	10 a 0	21 a 0			NW	Clear.
19	15 a 0	14 a 0			N by E	Cloudy.
20	6 a 0	16 a 0			N by E	Clear.
21	6 a 0	29 a 0			N by E	Clear.
22	27 a 0	26 a 0	8		NE	Cloudy, with a great quantity of ſnow.
23	25 a 0	29 a 0			NE	Cloudy.
24	12 a 0	29 a 0			N	Cloudy, with a thick fog.
25	26 a 0	27 a 0	4		NE	Cloudy, with ſnow.
26	26 a 0	31 a 0			W	Clear.
27	25 a 0	28 a 0			W	Clear.
*28	24 a 0	40 a 0			SW	Clear.
29	32 a 0	41 a 0			NW	Clear.
30	31 a 0	40 a 0		Rain.	N by E	Cloudy, with rain.
31	40 a 0	42 a 0		Rain.	N by E	Cloudy, with rain.

* This day and yeſterday the ice on the river began to be rotten, and croſſing to be hazardous.

FOR

FOR APRIL, 1779.

Days.	Thermometer. Morning	Thermometer. Evening.	Snow.	Rain.	Winds.	Remarks.
	°	°	Inch.			
1	40 a 0	46 a 0		Rain.	SW	Cloudy, with a thick fog and rain.
2	41 a 0	45 a 0			SW	Cloudy, with a thick fog and rain.
3	30 a 0	51 a 0		Rain.	NE	Cloudy, wind SW in the evening.
4	35 a 0	41 a 0			W	Clear.
5	36 a 0	42 a 0			W	Clear.
6	37 a 0	38 a 0		Rain.	SW	Cloudy, with a little rain.
7	32 a 0	36 a 0			W	Cloudy.
8	32 a 0	49 a 0			SW	Cloudy.
9	42 a 0	44 a 0			S	Cloudy.
10	40 a 0	42 a 0			E	Cloudy.
11	35 a 0	44 a 0		Rain.	E	Cloudy.
12	42 a 0	45 a 0		Rain.	E	Cloudy, a very heavy rain.
13	39 a 0	40 a 0			W	Clear.
14	31 a 0	45 a 0			NE	Clear.
15	46 a 0	50 a 0			NW	Clear.

It is worth obſerving, that the great quantity of ſnow on the ground has, from the 29th of March to the 4th of April, been chiefly melted away by the heat of the ſun, for the rain during that period has been very inconſiderable.

No *refoulement* of the river this ſpring; the ice gave way in the middle of the river firſt, and the opening has been growing larger and larger every day ſince the 28th of March, and is this day, the 4th of April, very large, although the ice remains faſt on the border of the river.

METEOROLOGICAL JOURNAL

KEPT AT THE HOUSE OF

THE ROYAL SOCIETY,

BY ORDER OF THE

PRESIDENT AND COUNCIL.

METEOROLOGICAL JOURNAL
for January 1778.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Jan. 1	8	0	33,5	48,5	29,66	0,116	NNW	3	Rain, violent wind last n.
	2	0	32,5	37,0	29,85		N by W	3	Fair.
2	8	0	33,5	35,5	30,26		NNE	1	Cloudy.
	2	0	36,0	37,0	30,38		NW	1	Fine.
3	8	0	26,5	33,5	30,55		NNE	1	Fair and frosty.
	2	0	32,0	34,0	30,51		N by W	1	Fine.
4	8	0	25,0	31,5	30,49		WNW	1	Foggy and frosty.
	2	0	28,0	32,0	30,45		NW	1	Fine.
5	8	0	32,0	32,5	30,28		SSW	1	Foggy.
	2	0	37,0	32,0	30,26		SW	1	Fine.
6	8	0	37,0	36,0	30,23		NE	1	Fair.
	2	0	42,0	38,0	30,27		NE	1	Cloudy and rainy.
7	8	0	31,0	35,5	30,45		NE	1	Foggy.
	2	0	37,5	36,5	30,54		NE	1	Fine.
8	8	0	29,0	34,0	30,55		ENE	1	Foggy and frosty.
	2	0	34,0	34,5	30,61		NE	1	Fine.
9	8	0	28,0	32,0	30,50		N by E	1	Frosty.
	2	0	37,0	34,5	30,47		NE	1	Fine.
10	8	0	36,5	36,0	30,40		NE	1	Cloudy.
	2	0	38,0	37,0	30,42		NE	1	Cloudy.
11	8	0	29,0	34,0	30,41		NE	1	Foggy.
	2	0	35,0	35,5	30,39		ENE	1	Fair.
12	8	0	26,0	32,0	30,44		NE	1	Fair and frosty.
	2	0	36,0	35,0	30,42		N by E	1	Fine.
13	8	0	36,0	35,0	30,41		N by E	1	Foggy.
	2	0	40,0	37,0	30,42		NE	1	Cloudy and rainy.
14	8	0	33,0	36,0	30,33		N by W	1	Foggy.
	2	0	37,0	37,5	30,35		NW	1	Fair.
15	8	0	32,5	36,0	30,28		NE	1	Fine.
	2	0	34,5	37,5	30,29		NE	1	Fine.
16	8	0	32,5	35,0	30,19		NNE	1	Foggy.
	2	0	35,0	36,0	30,21		NE	1	Foggy and snow.

METEOROLOGICAL JOURNAL for January 1779.

	Time.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Jan. 17	8	0	34,0	36,0	30,30		NE	1	Foggy.
	2	0	36,0	36,5	30,27		NNE	1	Cloudy.
18	8	0	32,0	35,0	30,22		S by W	1	Cloudy.
	2	0	34,0	36,0	30,28		SW	1	Rain.
19	8	0	35,0	35,0	30,27		SSW	1	Foggy.
	2	0	45,0	37,0	30,37		SW	1	Foggy.
20	8	0	38,0	39,0	30,49	0,027	S	1	Foggy.
	2	0	45,0	48,5	30,52		SW	1	Foggy.
21	8	0	38,0	40,0	30,35		S by E	1	Cloudy.
	2	0	44,5	42,5	30,29		SE	1	Fine.
22	8	0	41,0	42,5	30,20		S by W	1	Fair.
	2	0	49,0	44,5	30,24		SW	1	Fine.
23	8	0	40,0	42,0	30,37		W	1	Fair.
	2	0	48,0	44,5	30,34		SW	1	Fine.
24	8	0	37,0	44,0	30,33		S by E	1	Cloudy.
	2	0	49,0	43,5	30,24		S by W	1	Fine.
25	8	0	39,0	42,0	29,95		S by E	1	Fair.
	2	0	45,0	43,0	29,93		SE	1	Rainy and cloudy.
26	8	0	39,0	41,0	29,87	0,020	ESE	1	Fair.
	2	0	45,5	42,5	29,81		ESE	1	Fine.
27	8	0	41,0	42,5	29,66		S by E	1	Cloudy.
	2	0	44,5	43,5	29,63		S by E	1	Rainy.
28	8	0	39,0	41,5	29,86	0,053	WNW	1	Cloudy.
	2	0	41,0	42,5	29,99		NW	1	Cloudy.
29	8	0	31,5	39,0	30,15		SSW	1	Fair.
	2	0	43,0	41,5	30,25		SW	1	Fine.
30	8	0	43,0	40,0	30,24		SW	1	Cloudy.
	2	0	50,0	43,0	30,31		SW	1	Fine.
31	8	0	45,0	45,0	30,39		WSW	1	Cloudy.
	2	0	48,5	45,5	30,44		SW	1	Fine.

METEOROLOGICAL JOURNAL for February 1779.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
					Inches.	Inch.	Points	Str.	
Feb. 1	8	0	43,0	46,0	30,41		W	1	Fair.
	2	0	48,0	47,0	30,45		NW	1	Cloudy.
2	8	0	41,0	46,0	30,42		SSW	1	Fair.
	2	0	50,0	47,0	30,41		SW	1	Fine.
3	8	0	37,0	44,0	30,31		ENE	1	Foggy.
	2	0	45,0	45,0	30,26		NW	1	Fine.
4	8	0	46,0	47,0	30,14		W by N	1	Fair.
	2	0	50,0	48,0	30,14		NW	1	Fine.
5	8	0	48,0	49,5	30,07		WSW	2	Cloudy.
	2	0	51,0	49,5	30,10		WSW	2	Fine.
6	8	0	47,0	49,0	30,12		SW	2	Fine.
	2	0	55,0	52,5	30,14		SW	2	Fine.
7	8	0	50,0	51,0	30,32		WSW	1	Cloudy.
	2	0	55,5	53,5	30,32		WSW	1	Cloudy.
8	8	0	47,0	52,0	30,39		W by S	1	Cloudy.
	2	0	52,5	53,0	30,44		SW	1	Fine.
9	8	0	48,0	51,5	30,36		ENE	1	Cloudy.
	2	0	52,5	53,5	30,34		ENE	1	Cloudy.
10	8	0	45,0	49,5	30,11		SW	1	Cloudy.
	2	0	47,5	50,5	30,11		S by W	1	Cloudy.
11	8	0	42,0	48,5	30,14		SE	1	Fine.
	2	0	52,0	50,0	30,12		SE	1	Fine.
12	8	0	45,0	47,0	29,85		SSE	1	Fair.
	2	0	51,0	48,0	29,84		SW	1	Fine.
13	8	0	49,0	48,0	29,85		SW	2	Cloudy.
	2	0	53,0	51,0	29,96		SW	2	Fine, much wind.
14	8	0	48,0	50,0	29,98	0,164	SW	1	Fair.
	2	0	53,0	51,5	30,10		SW	1	Fine.
15	8	0	40,0	47,0	30,44	0,025	SSW	1	Cloudy.
	2	0	52,0	49,0	30,44		SW	1	Fine.
16	8	0	47,0	48,0	30,27		SSW	1	Cloudy.
	2	0	53,0	50,0	30,39		SW	1	Fine.

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for February 1779.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
					Inches.	Inch.	Points.	Str.	
Feb. 17	8	0	45,0	47,5	30,46		SW	1	Cloudy.
	2	0	57,0	50,0	30,51		SW	1	Fine.
18	8	0	44,5	49,0	30,46		SSW	1	Fine.
	2	0	55,5	51,0	30,44		S by W	1	Fine.
19	8	0	41,0	48,0	30,44		SW	1	Fine.
	2	0	54,5	50,5	30,53		SW	1	Fine.
20	8	0	38,0	47,0	30,52		SSW	1	Foggy.
	2	0	47,5	49,0	30,50		S by W	1	Fine.
21	8	0	40,0	45,0	30,34		SSW	1	Cloudy.
	2	0	46,0	47,0	30,29		WSW	1	Cloudy.
22	8	0	40,5	45,0	30,22		SW	1	Cloudy.
	2	0	40,0	46,5	30,21		SW	1	Fine.
23	8	0	40,0	44,5	30,17		S by W	1	Fair.
	2	0	50,0	45,0	30,14		SW	1	Fine.
24	8	0	43,0	46,0	30,10		E by N	1	Fine.
	2	0	51,0	47,0	30,16		NE	1	Cloudy.
25	8	0	43,0	47,0	30,21	0,050	SSE	1	Cloudy.
	2	0	52,5	49,5	30,22		S by E	1	Fine.
26	8	0	47,0	49,0	30,23		S by E	1	Fair.
	2	0	59,0	52,0	30,26		SW	1	Cloudy.
27	8	0	48,0	50,0	30,17		SW	1	Foggy.
	2	0	60,5	53,0	30,53		SW	1	Fine.
28	8	0	43,0	50,0	30,19		SSE	1	Fair.
	2	0	58,0	52,0	30,38		NE	1	Foggy.

METEOROLOGICAL JOURNAL for March 1779.

	Time.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 1	7	0	39,5	49,0	30,16		SSW	1	Fair.
	2	0	59,0	51,0	30,17		SSW	1	Fine.
2	7	0	44,0	49,0	30,27		NNW	1	Fair.
	2	0	50,5	50,0	30,29		W by N	1	Fine.
3	7	0	34,5	45,0	30,34		SSW	1	Fine.
	2	0	50,0	47,5	30,37		SW	1	Fair.
4	7	0	45,0	47,0	30,49	0,051	S by E	1	Rainy.
	2	0	50,5	49,0	30,56		S by W	1	Cloudy.
5	7	0	37,0	47,0	30,60		ENE	1	Foggy.
	2	0	50,0	50,0	30,63		NE	1	Fine.
6	7	0	36,0	43,5	30,57		NNE	1	Foggy.
	2	0	51,0	46,5	30,61		NW	1	Fine.
7	7	0	40,0	44,0	30,48		NE	1	Fair.
	2	0	47,5	46,5	30,45		ENE	1	Cloudy.
8	7	0	39,0	44,0	30,41		ENE	1	Fine.
	2	0	52,0	48,0	30,40		NE	1	Fine.
9	7	0	37,0	43,5	30,32		NE	1	Fine.
	2	0	52,0	48,0	30,32		NE	1	Fine.
10	7	0	41,0	44,5	30,24		ENE	1	Cloudy.
	2	0	50,0	46,5	30,17		NE	1	Fine.
11	7	0	38,5	44,5	30,06		E	1	Fair.
	2	0	50,0	46,0	30,00		ENE	1	Fine.
12	7	0	33,0	42,0	29,93		ENE	1	Fine.
	2	0	54,5	45,5	29,95		NE	1	Fine.
13	7	0	37,0	44,0	30,14		SSW	1	Fine.
	2	0	50,0	48,0	30,14		SW	1	Fine.
14	7	0	41,0	45,0	30,15		SW	1	Fair.
	2	0	50,0	48,0	30,09		SW	1	Rainy.
15	7	0	44,0	48,0	29,84	0,052	WNW	1	Fine.
	2	0	52,0	50,0	29,96		NW	1	Cloudy.
16	7	0	38,5	45,0	30,06		NNE	1	Cloudy.
	2	0	47,0	47,0	30,05		NE	1	Fine.

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	Time. H. M.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
Mar. 17	7	0	40,0	44,5	29,95		S by W	1	Cloudy.
	2	0	50,0	47,0	29,89		SW	1	Fine.
18	7	0	44,0	45,5	29,64	0,381	SSE	1	Rainy.
	2	0	52,0	49,0	29,63		SE	1	Fair.
19	7	0	43,0	48,5	29,64		W by S	1	Fine.
	2	0	54,5	51,0	29,72		W by S	1	Fine.
20	7	0	50,0	51,0	29,80	0,029	S by W	1	Cloudy.
	2	0	56,0	54,0	29,79		SSW	1	Fine.
21	7	0	46,0	52,0	29,72		WSW	2	Cloudy.
	2	0	49,5	53,0	29,79		W by S	2	Showery with sun-shine.
22	7	0	45,0	49,0	30,27	0,010	W by S	1	Fine.
	2	0	55,0	52,0	30,34		SW	1	Fair.
23	7	0	49,0	51,0	30,33		SW	1	Fair.
	2	0	57,0	52,0	30,36		SW	1	Fine.
24	7	0	47,0	49,0	30,32		SSW	1	Fine.
	2	0	62,0	53,0	30,35		SW	1	Fine.
25	7	0	39,5	49,5	30,31		NNE	1	Fine.
	2	0	60,0	54,0	30,29		NW	1	Fine.
26	7	0	42,0	50,0	30,15		NW	1	Fine.
	2	0	61,0	53,0	30,07		NW	1	Fine.
27	7	0	42,0	53,0	30,08		NW	1	Fine.
	2	0	60,0	56,0	30,17		NW	1	Fine.
28	7	0	42,0	50,0	30,11		NW	1	Fine.
	2	0	73,0	52,0	30,48		NE	1	Fine.
29	7	0	42,0	47,0	30,40		NE	1	Fine.
	2	0	73,0	49,0	30,43		N by E	1	Fine.
30	7	0	38,0	35,0	30,35		NE	1	Fine.
	2	0	51,0	48,0	30,29		SE	1	Fine.
31	7	0	36,0	44,0	30,30		NE	1	Fine
	2	0	57,0	48,5	30,29		NE	1	Fine.

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METEOROLOGICAL JOURNAL
for April 1779.

	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 1	7	0	43,0	47,0	30,44		NE	1	Cloudy.
	2	0	50,0	49,0	30,48		NE	1	Cloudy.
2	7	0	38,0	44,0	30,54		NE	1	Fine.
	2	0	56,0	51,0	30,59		NE	1	Fine.
3	7	0	38,0	44,5	30,49		NE	1	Fine.
	2	0	62,0	58,0	30,43		NE	1	Fine.
4	7	0	40,0	46,5	30,25		NE	1	Fine.
	2	0	65,0	51,0	30,18		WSW	1	Fine.
5	7	0	47,0	50,0	30,09		NE	1	Fair.
	2	0	67,5	55,0	30,05		SSE	1	Fine.
6	7	0	47,0	51,0	30,03		SE	1	Fine.
	2	0	67,0	57,0	30,25		SE	1	Fine.
7	7	0	51,0	52,0	30,19	0,051	SSW	1	Cloudy.
	2	0	60,5	57,0	30,14		S by W	2	Cloudy.
8	7	0	53,0	54,0	29,84		S by W	2	Fine.
	2	0	63,0	59,0	29,73		SW	2	Cloudy and rain.
9	7	0	43,5	53,5	29,77	0,175	WSW	2	Fine.
	2	0	54,0	55,0	29,81		SW	2	Rainy.
10	7	0	44,5	49,5	29,97	0,027	W by S	1	Fair.
	2	0	52,0	54,0	29,99		SW	1	Cloudy with little rain.
11	7	0	53,0	51,0	30,00		SW	2	Fair.
	2	0	64,0	56,5	30,03		SW	2	Fine.
12	7	0	51,0	52,0	30,10		S by W	1	Cloudy.
	2	0	62,0	58,0	30,14		SW	1	Cloudy.
13	7	0	52,0	54,0	30,12	0,010	SW	1	Fair.
	2	0	66,0	60,0	30,12		SW	1	Fine.
14	7	0	48,0	56,5	30,05		SW	1	Fair.
	2	0	71,0	61,0	29,99		SW	1	Fine.
15	7	0	52,0	58,0	29,85		ENE	1	Fine.
	2	0	78,0	64,0	29,86		SW	1	Fine.
16	7	0	51,5	60,0	29,90		ENE	1	Cloudy.
	2	0	55,0	60,0	29,96		NE	1	Cloudy.

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	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Apr. 17	7	0	48,0	53,0	30,19		E by S	1	Cloudy.
	2	0	55,0	55,5	30,20		E by S	2	Cloudy.
18	7	0	51,5	52,5	30,06		ESE	1	Fine.
	2	0	69,0	60,5	29,99		E	1	Fine.
19	7	0	57,5	59,0	29,96	0,016	SSW	1	Cloudy.
	2	0	68,5	62,5	30,03		SW	1	Fine.
20	7	0	54,0	58,0	30,09		SSW	1	Fine.
	2	0	66,0	62,0	30,07		SW	2	Fine.
21	7	0	53,0	59,0	29,82	0,100	W by S	2	Fine.
	2	0	61,0	60,0	29,78		S by W	2	Fine.
22	7	0	55,0	57,0	29,81	0,033	SSW	3	Cloudy.
	2	0	60,0	57,5	29,77		SW	2	Cloudy.
23	7	0	47,0	54,0	29,66	0,210	W by S	2	Cloudy and rain.
	2	0	56,0	56,5	29,74		SSW	3	Cloudy.
24	7	0	49,0	53,0	29,60	0,245	SW	2	Fair.
	2	0	45,0	54,0	29,58		SW	2	Showery.
25	7	0	45,0	50,0	29,78	0,218	W by N	2	Fine.
	2	0	57,0	52,5	29,88		SW	2	Fine.
26	7	0	49,0	52,0	29,43	0,094	W by S	2	Fine.
	2	0	58,0	54,5	29,55		NW	2	Fine.
27	7	0	46,0	51,0	29,60		SW	2	Fine.
	2	0	47,0	52,5	29,65		NW	2	Showery.
28	7	0	45,0	50,0	29,57	0,045	WSW	2	Fine.
	2	0	55,0	52,0	29,59		SW	2	Fine.
29	7	0	50,0	50,5	29,65		WSW	2	Cloudy.
	2	0	56,5	54,0	29,69		W by S	2	Fine.
30	7	0	45,0	50,0	29,74		NE	1	Fine.
	2	0	59,0	53,5	29,79		E by N	1	Fine.

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METEOROLOGICAL JOURNAL
for May 1779.

	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
May 1	7	0	47,0	52,0	29,52	0,342	NNE	1	Rainy.
	2	0	48,0	52,0	29,51		NNW	1	Rainy.
2	7	0	41,5	48,0	29,80	0,021	NNE	1	Fine.
	2	0	55,[illegible]	51,5	29,81		SSW	1	Fine.
3	7	0	43,0	48,5	29,68		N by E	1	Rainy.
	2	0	45,0	50,0	29,65		N by E	1	Rainy.
4	7	0	42,0	45,0	29,64	0,240	N by W	1	Rainy.
	2	0	50,0	48,0	29,69		NW	1	Cloudy.
5	7	0	42,0	45,0	29,65		NNW	1	Cloudy.
	2	0	45,0	48,0	29,62		NW	1	Rainy.
6	7	0	47,0	48,0	29,82	0,202	SSW	1	Fair.
	2	0	61,0	52,0	29,80		SW	1	Cloudy.
7	7	0	53,0	53,5	29,71	0,015	SSW	1	Fair.
	2	0	60,0	55,0	29,74		SW	1	Showery, but sun-shine.
8	7	0	51,0	53,0	29,67	0,094	ESE	1	Fine.
	2	0	56,0	55,0	29,61		ENE	1	Showery.
9	7	0	53,0	55,0	29,63	0,083	SSW	1	Cloudy.
	2	0	65,0	58,5	29,77		SSW	1	Fine.
10	7	0	51,0	51,5	29,69		ENE	1	Fine.
	2	0	70,0	68,5	29,64		SW	1	Fine.
11	7	0	52,0	57,0	29,67	0,085	WSW	1	Fine.
	2	0	65,0	61,0	29,78		SSW	1	Fine.
12	7	0	49,0	57,5	29,87		SW	1	Fine.
	2	0	64,0	60,0	29,93		SSW	1	Fine.
13	7	0	55,5	58,0	29,91		W	1	Fine.
	2	0	68,0	61,5	29,96		WNW	1	Fine.
14	7	0	53,0	59,0	29,93		SSW	1	Fine.
	2	0	67,5	62,0	29,97		SSW	1	Fine.
15	7	0	58,0	60,0	29,92		SSW	1	Fair.
	2	0	67,0	63,0	29,93		W by S	1	Fair.
16	7	0	57,0	60,5	29,86	0,039	SSW	1	Rainy.
	2	0	60,0	62,5	29,86		SW	2	Cloudy and showery.

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	Time. H. M.		Therm. without	Therm. within.	Barom. Inches.	Rain. Inch.	Winds. Points.	Str.	Weather.
May 17	7	0	58,0	61,0	29,78	0,087	SSW	2	Cloudy.
	2	0	66,0	64,0	29,80		SSW	2	Fine.
18	7	0	59,0	62,0	29,83		SSW	2	Cloudy, with little sun-sh.
	2	0	58,5	62,0	29,78		SW	2	Rain.
19	7	0	53,5	60,0	29,78	0,315	WSW	2	Cloudy.
	2	0	63,5	61,5	29,81		WSW	2	Showery.
20	7	0	52,0	58,0	29,83	0,146	WSW	2	Cloudy.
	2	0	58,0	59,0	29,88		SW	2	Cloudy.
21	7	0	55,5	57,0	30,02	0,039	SW	1	Cloudy.
	2	0	60,0	59,5	30,06		SW	2	Cloudy.
22	7	0	55,0	58,0	30,16		NW	1	Fine.
	2	0	66,0	61,5	30,16		NW	1	Fine.
23	7	0	58,0	59,5	30,19		ENE	1	Fine.
	2	0	78,0	66,0	30,12		SE	1	Fine.
24	7	0	65,5	66,0	30,03		S by W	1	Fine.
	2	0	82,5	71,5	30,07		S by W	1	Fine.
25	7	0	63,0	60,0	30,20		ENE	1	Fine.
	2	0	80,0	73,5	30,20		NE	1	Fine.
26	7	0	54,5	61,0	30,12		NE	1	Cloudy.
	2	0	75,0	70,0	30,12		E	1	Fine.
27	7	0	57,5	64,0	30,09		NE	1	Fair.
	2	0	70,0	67,5	30,12		NE	1	Fine.
28	7	0	54,5	63,0	30,03		NE	1	Fair.
	2	0	74,5	68,0	30,02		NE	1	Fine.
29	7	0	64,5	65,5	29,96		ENE	1	Fine.
	2	0	77,5	79,0	30,04		E by N	1	Fine.
30	7	0	56,0	64,0	30,22		ENE	1	Fair.
	2	0	62,0	65,0	30,27		NE	1	Cloudy.
31	7	0	56,5	57,5	30,19		NE	1	Fine.
	2	0	66,5	62,0	30,20		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 1	7	0	56,0	61,0	30,08		NE	1	Fine.
	2	0	67,5	63,0	30,02		E by N	1	Fine.
2	7	0	53,5	57,5	30,02		N by E	1	Fine.
	2	0	70,0	62,0	30,14		NE	1	Fine.
3	7	0	56,0	58,0	29,98		NE	1	Fine.
	2	0	67,0	63,5	30,04		NE	1	Fine.
4	7	0	54,0	56,5	29,97		S by E	1	Fine.
	2	0	70,0	63,5	29,95		NW	1	Fine.
5	7	0	54,0	61,0	29,90		NW	1	Fine.
	2	0	67,0	63,5	29,87		N by E	1	Fine.
6	7	0	56,0	51,5	29,90		ENE	1	Fine.
	2	0	71,0	65,0	29,89		SE	1	Fine.
7	7	0	62,0	63,0	29,78		ESE	1	Fine.
	2	0	73,5	68,0	29,73		SSE	1	Fine.
8	7	0	61,0	65,0	29,54	0,086	SSW	1	Fine.
	2	0	70,0	68,0	29,48		SW	1	Fine.
9	7	0	57,0	65,0	29,48	0,028	SW	2	Cloudy.
	2	0	68,0	66,0	29,51		SW	2	Cloudy.
10	7	0	59,5	64,5	29,66	0,040	SSW	1	Cloudy.
	2	0	62,0	65,0	29,72		SW	1	Cloudy.
11	7	0	53,0	61,5	29,65	0,303	N by W	1	Rainy.
	2	0	53,0	62,0	29,64		NE	2	Rainy.
12	7	0	55,5	61,0	29,50	1,198	ENE	1	Cloudy.
	2	0	70,0	65,0	29,55		S by E	1	Fine.
13	7	0	58,0	61,0	29,53		W by S	1	Fair.
	2	0	72,0	65,5	29,51		S by E	1	Fine.
14	7	0	58,0	63,5	29,57		NE	1	Fair.
	2	0	71,0	66,5	29,64		NW	1	Fair.
15	7	0	59,5	64,0	29,90		NNE	1	Fine.
	2	0	71,5	69,0	30,00		NNE	1	Fine.
16	7	0	53,5	61,0	30,11		N by W	2	Cloudy.
	2	0	62,0	63,0	30,15		N	2	Fair.

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	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
June 17	7	0	54,5	60,0	30,15		N by W	1	Cloudy.
	2	0	72,0	63,0	30,17		NW	1	Fine.
18	7	0	56,0	59,0	30,05		SW	1	Fair.
	2	0	75,0	65,0	29,97		NW	1	Fine.
19	7	0	55,0	61,0	29,97		NW	1	Fine.
	2	0	61,5	62,0	29,97		WSW	1	Fine.
20	7	0	52,5	59,5	30,10	0,058	NW	1	Fine.
	2	0	62,5	61,0	30,12		NW	1	Fine.
21	7	0	58,0	60,0	30,16		W by N	1	Fine.
	2	0	71,0	64,0	30,16		WSW	1	Fine.
22	7	0	56,5	61,5	30,15	0,120	NNW	1	Rainy.
	2	0	61,0	63,0	30,22		NW	1	Cloudy.
23	7	0	57,0	60,5	30,21		N by E	1	Fine.
	2	0	62,5	64,0	30,22		NE	1	Cloudy.
24	7	0	56,5	60,0	30,13		NNE	1	Fine.
	2	0	63,0	63,0	30,13		NW	1	Cloudy.
25	7	0	55,0	60,0	29,92	0,385	N by E	1	Rainy.
	2	0	60,0	62,0	29,89		NNE	1	Showery.
26	7	0	58,0	61,0	29,92	0,258	NE	2	Fine.
	2	0	69,0	65,0	29,99		NE	2	Fine.
27	7	0	55,0	58,5	30,02		N by E	2	Fair.
	2	0	65,5	63,5	30,01		ENE	2	Fine.
28	7	0	57,0	60,0	30,02		ENE	1	Cloudy.
	2	0	68,0	64,0	30,01		NNE	1	Fine.
29	7	0	54,5	60,0	30,06		N by E	1	Fair.
	2	0	69,0	66,0	30,12		N by E	1	Fine.
30	7	0	57,5	61,0	30,22		SSW	1	Fine.
	2	0	72,0	65,5	30,21		S by W	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 1	7	0	62,5	64,0	30,08		SSW	1	Fine.
	2	0	73,0	68,0	30,00		SW	1	Fine.
2	7	0	59,0	64,5	29,85		N by W	1	Fine.
	2	0	76,0	69,0	29,81		NW	1	Fine.
3	7	0	61,0	65,0	29,60	0,160	NW	1	Rainy.
	2	0	63,0	67,0	29,50		NW	1	Rainy.
4	7	0	61,0	64,0	29,47	0,500	SSE	1	Fine.
	2	0	68,0	65,0	29,35		SE	1	Rainy.
5	7	0	59,0	64,5	29,52	1,000	S by W	2	Fine.
	2	0	65,0	66,0	29,35		SW	2	Showery.
6	7	0	57,0	63,0	29,30	0,228	SW	1	Rainy.
	2	0	63,0	65,0	29,48		SW	1	Cloudy.
7	7	0	60,0	64,0	29,76	0,160	N by E	1	Fair.
	2	0	66,5	66,0	29,89		NNW	1	Showery.
8	7	0	60,0	64,5	30,14	0,020	NNW	1	Fine.
	2	0	73,5	67,5	30,17		N	1	Fine.
9	7	0	60,0	66,5	30,30		NNE	1	Fine.
	2	0	78,0	70,0	30,28		NE	1	Fine.
10	7	0	64,0	67,0	30,28		SSW	1	Fine.
	2	0	80,0	73,5	30,29		NW	1	Fine.
11	7	0	69,0	65,0	30,34		NW	1	Fine.
	2	0	82,0	76,5	30,38		NNW	1	Fine.
12	7	0	65,0	73,5	30,41		N by W	1	Fair.
	2	0	81,0	76,5	30,42		NW	1	Fine.
13	7	0	65,5	73,5	30,33		NW	1	Fine.
	2	0	84,0	80,5	30,29		NW	1	Fine.
14	7	0	66,0	69,0	30,27		ENE	1	Fine.
	2	0	77,0	77,0	30,31		NE	1	Fine.
15	7	0	66,0	69,0	30,34		ESE	1	Fine.
	2	0	74,0	76,5	30,38		SE	1	Fine.
16	7	0	69,0	74,0	30,29		SSW	1	Fine.
	2	0	82,0	81,0	30,34		NE	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
July 17	7	0	64,5	67,0	30,25		ESE	1	Fine.
	2	0	74,0	72,0	30,21		SE	1	Fine.
18	7	0	67,5	68,0	30,11		E by S	1	Fine.
	2	0	85,0	75,0	30,09		SSE	1	Fine.
19	7	0	63,0	72,0	30,00		SW	1	Fair.
	2	0	77,5	76,0	29.95		SW	1	Cloudy.
20	7	0	64,0	70,0	29,88		SSW	1	Fine.
	2	0	76,0	74.5	29,84		SW	1	Fine.
21	7	0	63,5	70,5	29,72		NNW	1	Fair.
	2	0	62,0	70,0	29,54		NW	1	Rainy.
22	7	0	62,0	68,5	29,58	2,867	SSW	1	Fine.
	2	0	64,0	68,5	29,52		SE	1	Showery.
23	7	0	61,0	66,0	29,33	0,853	SE	1	Rainy.
	2	0	60,5	68,0	29,32		NE	1	Rainy.
24	7	0	61,0	66,0	29,63	0,140	NW	1	Fair.
	2	0	70 0	68,5	29,66		N by W	1	Fine.
25	7	0	64,5	68,0	29,74		SSW	1	Fair.
	2	0	74,0	70,5	29,71		NW	1	Fine.
26	7	0	60,0	67,5	29,76	0,269	NNE	1	Fair.
	2	0	71,5	70,0	29,84		NE	1	Fine.
27	7	0	59,0	67,0	29,92		S	1	Fine.
	2	0	73.0	70,0	29,97		SW	1	Cloudy.
28	7	0	64,0	66.0	29,85		WSW	1	Cloudy.
	2	0	78.0	74,0	29,88		SW	1	Fine.
29	7	0	67,0	71,0	29,81	0,029	SW	1	Cloudy.
	2	0	76,5	74,0	29,78		SW	1	Fair.
30	7	0	64,0	69,0	29,67		SSW	1	Cloudy.
	2	0	73,0	72,0	29,69		WSW	1	Showery.
31	7	0	62,0	69,0	29,79	0,175	NW	2	Cloudy.
	2	0	66,0	69,0	29,89		WSW	2	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 1	7	0	59,5	67,0	30,09		S by W	1	Fine.
	2	0	75,0	70,0	30,08		SW	1	Fine.
2	7	0	62,0	68,0	30,03		SSW	1	Fine.
	2	0	79,0	72,0	29,96		SSW	1	Fine.
3	7	0	60,0	69,0	30,01	0,044	SW	1	Fine.
	2	0	73 5	72,5	30,08		NW	1	Fine.
4	7	0	63,5	69,5	29,97		SSW	1	Cloudy.
	2	0	67,0	70,0	29,90		SSW	1	Rain.
5	7	0	61,0	67,0	29,82	0,110	S by E	1	Rainy.
	2	0	69,0	69,5	29,78		SSW	1	Showery.
6	7	0	58,0	66,0	29,70	0,054	NNW	1	Rainy.
	2	0	57,0	65,5	29,67		NNW	1	Rainy.
7	7	0	55,0	62,0	29,64	0,712	NW	1	Rainy.
	2	0	64,0	66,0	29,68		NW	1	Rainy.
8	7	0	63,0	66,0	29,85	0,094	N by W	1	Fine.
	2	0	72,5	69,0	29,88		N by W	1	Fine.
9	7	0	60,5	66,0	29,86		NW	1	Cloudy.
	2	0	68,0	68,5	29,91		NW	1	Cloudy.
10	7	0	63,5	67,0	30,05		NNW	1	Cloudy.
	2	0	72,0	70,5	30,08		N by W	1	Cloudy.
11	7	0	62,0	68,0	30,08		ENE	1	Cloudy.
	2	0	76,0	72,5	30,11		NNE	1	Fine.
12	7	0	63,0	69,5	30,08		SSE	1	Cloudy.
	2	0	74,5	72,0	30,08		N	1	Fine.
13	7	0	63,0	69,5	30,12		S by W	1	Fair.
	2	0	75,0	72,0	30,09		S by W	1	Fine.
14	7	0	64,0	70,5	30,12		E by N	1	Fair.
	2	0	77,5	73,5	30,11		E by S	1	Fine.
15	7	0	65,0	67,0	30,01		SE	1	Cloudy.
	2	0	81,0	77,0	30,01		SSE	1	Fine.
16	7	0	62,0	71,0	30,05		S by E	1	Fair.
	2	0	77,0	73,0	30,07		SE	1	Fine.

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	Time.		Therm without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Aug. 17	7	0	62,0	69,0	30,09		NNE	1	Fine.
	2	0	74,0	73,5	30,13		NE	1	Fine.
18	7	0	60,0	67,5	30,17		NE	1	Fine.
	2	0	74,0	73,5	30,22		NE	1	Fine.
19	7	0	61,5	67,0	30,22		NE	1	Fine.
	2	0	74,5	72,5	30,27		SW	1	Fine.
20	7	0	61,5	67,0	30,27		NNE	1	Fair.
	2	0	75,0	70,0	30,29		NNE	1	Fine.
21	7	0	67,0	70,0	30,28		ENE	1	Fine.
	2	0	7 80	72,0	30,29		NE	1	Fine.
22	7	0	65,0	67,0	30,18		NNE	1	Fine.
	2	0	78,0	73,5	30,14		ENE	1	Fine.
23	7	0	63,5	69,5	30,04		N by E	1	Cloudy.
	2	0	65,0	72,0	30,04		NE	1	Rainy.
24	7	0	59,0	67,0	30,07	0,055	N by W	1	Fine.
	2	0	70,0	69,5	30,08		NW	1	Fine.
25	7	0	57,0	65,5	30,13		NE	1	Fair.
	2	0	72,0	68,5	30,10		ENE	1	Fine.
26	7	0	60,5	66,0	30,09		ENE	1	Cloudy.
	2	0	75,0	71,0	30,13		NNE	1	Fine.
27	7	0	62,5	65,5	30,07		E by S	1	Fair.
	2	0	78,0	73,0	30,15		SE	1	Fine.
28	7	0	62,5	67,5	30,18		SE	1	Fine.
	2	0	78,5	72,5	30,20		E	1	Fine.
29	7	0	65,5	71,0	30,23		NE	1	Fine.
	2	0	84,0	75,0	30,22		NE	1	Fine.
30	7	0	64,0	66,5	30,04		ENE	1	Fine.
	2	0	82,0	77,0	30,05		NE	1	Fine.
31	7	0	68,5	71,0	29,92		S	1	Fine.
	2	0	84,5	79,0	29,97		S	1	Fine.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 1	7	0	61,0	72,0	29,94		NE	1	Fine.
	2	0	79,5	75,5	29,83		ENE	1	Fine.
2	7	0	63,0	73,0	29,80	0,033	WSW	2	Fine.
	2	0	72,0	73,5	29,90		W by S	1	Fine.
3	7	0	59,0	70,0	29,99		NNW	2	Fine.
	2	0	74,5	72,0	29,98		SSW	2	Fine.
4	7	0	63,0	69,0	29,91	0,015	SSW	1	Cloudy.
	2	0	64,0	71,0	29,96		SW	1	Rainy.
5	7	0	57,0	66,0	29,76	0,095	SE	1	Cloudy.
	2	0	67,5	67,0	29,71		S by E	1	Cloudy.
6	7	0	51,5	63,0	29,92		NW	1	Fine.
	2	0	66,0	65,0	30,04		NW	1	Cloudy.
7	7	0	59,0	63,0	30,03	0,017	WSW	2	Cloudy.
	2	0	70,0	68,0	30,04		SW	2	Fine.
8	7	0	63,0	65,5	30,00		W by S	2	Cloudy.
	2	0	72,0	69,0	30,07		NW	2	Fair.
9	7	0	55,0	65,5	30,12		NW	1	Fine.
	2	0	65,0	66,0	30,13		NW	1	Fine.
10	7	0	62,0	64,0	30,07		W by N	1	Fair.
	2	0	72,0	69,0	30,10		N by W	1	Fair.
11	7	0	59,5	64,0	30,17		E by N	1	Fine.
	2	0	73,5	78,0	30,15		S by E	1	Fine.
12	7	0	53,5	60,0	30,03		SE	1	Cloudy.
	2	0	72,0	66,5	29,98		W by N	1	Fine.
13	7	0	61,0	66,0	30,01		W by S	1	Fair.
	2	0	70,0	69,0	30,07		SW	1	Fine.
14	7	0	60,0	65,5	30,01		WNW	1	Cloudy.
	2	0	61,0	66,0	29,99		NW	1	Rainy.
15	7	0	53,0	63,0	29,94	0,316	WSW	1	Fine.
	2	0	68,0	66,0	30,04		NW	1	Fine.
16	7	0	62,0	64,0	30,11		W by S	1	Fair.
	2	0	72,0	69,0	30,20		WSW	1	Fine.

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	Time.		Therm. without	Therm within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Sept. 17	7	0	61,0	64,0	30,17		SW	2	Cloudy.
	2	0	69,0	68,0	30,14		SW	2	Fine.
18	7	0	61,5	65,0	29,88		SSW	2	Fine.
	2	0	68,0	64,0	29,73		SW	2	Cloudy.
19	7	0	55,0	62,0	29,79	0,259	WSW	1	Cloudy.
	2	0	58,5	63,0	29,77		WSW	2	Fine.
20	7	0	48,0	58,0	29,91	0,074	W by S	1	Fine.
	2	0	61,5	61,0	29,96		W by S	1	Fine.
21	7	0	52,0	58,0	29,95		SSE	1	Cloudy.
	2	0	65,0	62,0	29,82		SSW	1	Showery.
22	7	0	58,0	60,0	29,75		SW	1	Fair.
	2	0	67,0	65,0	29,78		SW	2	Fine.
23	7	0	56,0	62,0	29,69	0,140	SSW	1	Rainy.
	2	0	62,0	64,0	29,72		SW	1	Rainy.
24	7	0	67,0	63,0	29,56	0,945	SW	1	Rainy.
	2	0	67,0	65,0	29,61		SW	1	Fine.
25	7	0	64,0	65,0	29,54	0,030	SW	2	Cloudy.
	2	0	70,0	68,0	29,55		SW	1	Cloudy.
26	7	0	66,0	68,0	29,78	0,045	SW	1	Cloudy.
	2	0	69,0	68,0	29,78		SW	1	Cloudy.
27	7	0	59,0	66,0	29,72	0,227	SW	1	Cloudy.
	2	0	67,0	67,0	29,88		SW	1	Fine.
28	7	0	57,5	63,5	30,00		S by W	1	Fair.
	2	0	66,0	65,5	30,06		SW	1	Fine.
29	7	0	59,5	63,5	29,96	0,036	SSE	1	Fair.
	2	0	68,0	66,5	29,90		SE	1	Fine.
30	7	0	58,0	64,0	29,68		SE	1	Fine.
	2	0	67,5	66,5	29,69		SE	1	Rainy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 1	7	0	53,5	62,5	29,65	0,079	NNW	1	Cloudy.
	2	0	59,0	62,0	29,74		NW	1	Fine.
2	7	0	45,0	54,0	29,97		N by W	1	Fine.
	2	0	54,5	57,0	30,08		NW	1	Fine.
3	7	0	42,5	50,0	30,11		NNW	1	Fine.
	2	0	52,5	52,0	30,06		NNW	2	Fine.
4	7	0	36,0	48,0	30,22		WNW	1	Fair.
	2	0	55,0	50,0	30,31		NW	1	Fine.
5	7	0	39,5	43,5	30,36		NE	1	Fine.
	2	0	58,5	55,5	30,42		NE	1	Fine.
6	7	0	50,5	52,0	30,18	0,039	NNE	1	Fair.
	2	0	57,5	55,5	30,08		NE	1	Rainy.
7	7	0	51,5	53,0	29,86	0,010	SE	1	Cloudy.
	2	0	60,0	57,5	29,90		SW	1	Rainy.
8	7	0	41,0	52,0	30,14		SSW	1	Fair.
	2	0	57,0	55,0	30,17		SW	1	Fine.
9	7	0	42,0	50,5	30,03		NE	1	Foggy.
	2	0	60,5	58,0	29,96		ESE	1	Fine.
10	7	0	54,0	56,0	29,82	0,266	WSW	1	Rainy.
	2	0	57,0	58,0	29,90		NW	1	Cloudy.
11	7	0	44,5	53,0	30,11	0,048	SW	1	Fair.
	2	0	61,5	57,0	30,08		SW	1	Fine.
12	7	0	58,0	57,0	29,91	0,030	SSW	1	Cloudy.
	2	0	64,0	59,5	29,91		SW	1	Cloudy.
13	7	0	55,0	60,0	30,00	0,075	WSW	1	Cloudy.
	2	0	64,0	62,0	30,00		SSW	1	Fair.
14	7	0	58,5	61,5	29,84		SE	1	Fair.
	2	0	63,0	63,0	29,70		SE	1	Cloudy.
15	7	0	50,5	60,0	29,69	0,010	SSW	1	Fair.
	2	0	62,0	62,0	29,67		ESE	1	Cloudy.
16	7	0	54,0	68,0	29,40	0,209	SSE	3	Much rain and wind last n.
	2	0	56,0	59,5	29,40		S by W	2	Showery.

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	Time.		Therm. without	Therm. within	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Oct. 17	7	0	47,0	55,0	29,68	1,070	SW	1	Fine.
	2	0	58,5	57,0	29,87		SW	2	Fine.
18	7	0	54,0	55,0	29,88		SSW	2	Cloudy.
	2	0	61,5	68,0	29,79		S by W	2	Fine.
19	7	0	56,0	58,0	29,59		S by E	1	Fair.
	2	0	64,5	62,5	29,64		SW	2	Cloudy.
20	7	0	50,0	58,5	29,69	0,050	SSW	1	Fair.
	2	0	58,0	59,0	29,84		WSW	1	Fine.
21	7	0	42,0	52,5	29,96		NE	1	Foggy.
	2	0	54,0	55,5	29,85		E by S	1	Rainy.
22	7	0	47,0	53,0	29,97	0,562	NNE	1	Fair.
	2	0	55,0	54,5	30,06		N by E	1	Fair.
23	7	0	47,0	51,5	30,14		N by E	1	Fair.
	2	0	57,0	53,5	30,14		NE	1	Cloudy.
24	7	0	54,5	54,5	30,18		E by S	1	Cloudy.
	2	0	60,0	56,5	30,17		SE	1	Cloudy.
25	7	0	54,5	60,0	30,15		SW	1	Fair.
	2	0	64,0	62,0	30,07		WSW	1	Fair.
26	7	0	57,0	59,0	29,95		WSW	1	Fair.
	2	0	61,5	62,0	29,91		WSW	1	Rainy.
27	7	0	53,0	58,0	29,89	0,134	WSW	1	Rainy.
	2	0	55,5	58,0	29,95		SW	1	Fine.
28	7	0	56,0	55,0	29,69	0,130	SW	2	Cloudy.
	2	0	57,0	57,0	29,68		SW	2	Rainy.
29	7	0	47,5	53,0	30,06	0,221	WSW	1	Fair.
	2	0	55,5	56,0	30,12		NW	1	Fine.
30	7	0	55,0	56,0	30,12		NNW	1	Fair.
	2	0	62,0	59,0	30,21		NW	1	Fair.
31	7	0	54,0	57,0	30,35		N by W	1	Fair.
	2	0	57,5	58,5	30,35		SW	1	Cloudy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 1	8	0	52,5	56,5	30,30		SE	1	Foggy.
	2	0	59,0	58,0	30,24		SSE	1	Cloudy.
2	8	0	50,0	53,5	30,09		NNE	1	Foggy.
	2	0	57,0	56,5	30,09		NW	1	Cloudy.
3	8	0	51,5	54,5	30,19		SSW	1	Fair.
	2	0	55,0	55,5	30,21		SSE	1	Cloudy.
4	8	0	55,0	56,0	30,10	0,020	SW	1	Cloudy.
	2	0	55,5	58,0	30,18		NW	1	Fine.
5	8	0	52,0	56,5	30,11	0,062	WSW	1	Rainy.
	2	0	56,0	57,0	30,11		SW	1	Cloudy.
6	8	0	46,5	54,0	30,21		WSW	1	Fair.
	2	0	53,5	55,0	30,22		NW	1	Fine.
7	8	0	50,0	43,5	30,20		W by N	1	Fair.
	2	0	69,5	65,5	30,18		NNW	1	Cloudy.
8	8	0	52,0	55,0	30,24		W by S	1	Foggy.
	2	0	55,0	55,5	30,24		NW	1	Cloudy.
9	8	0	44,5	53,5	30,33		W by S	1	Fair.
	2	0	52,0	53,5	30,38		NW	1	Cloudy.
10	8	0	45,5	51,0	30,25	0,264	NNE	1	Cloudy.
	2	0	46,0	50,0	30,28		NW	1	Cloudy.
11	8	0	46,0	48,0	30,10		W by N	1	Fair.
	2	0	50,0	50,0	30,07		NW	1	Showery and cloudy.
12	8	0	44,5	46,5	29,92		SW	1	Rainy.
	2	0	52,5	49,5	29,76		WNW	1	Showery.
13	8	0	43,5	47,5	29,56	0,100	NW	1	Fair.
	2	0	45,0	48,0	29,44		SW	1	Cloudy.
14	8	0	32,0	42,0	29,28	1,145	W by N	2	Fair.
	2	0	40,5	42,5	29,22		NW	2	Rain and ſnow.
15	8	0	36,0	40,0	28,99	0,122	WNW	1	Fair.
	2	0	38,0	41,0	29,00		SW	1	Cloudy and rain.
16	8	0	34,0	39,0	29,39	0,122	WNW	1	Fair.
	2	0	41,5	41,0	29,45		WNW	1	Rainy.

METE-

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Nov. 17	8	0	37,0	39,5	29,58	0,018	NNW	1	Cloudy.
	2	0	40,0	40,0	29,52		NW	1	Cloudy.
18	8	0	32,0	38,0	29,58		NE	1	Cloudy.
	2	0	37,0	38,5	29,62		NW	1	Fine.
19	8	0	30,0	37,0	29,64		W by N	1	Foggy.
	2	0	40,5	39,0	29,58		W by N	1	Fine.
20	8	0	32,5	37,0	29,46		SSW	1	Cloudy.
	2	0	37,0	38,0	29,29		SW	1	Cloudy.
21	8	0	35,0	36,5	29,09	0,160	NW	1	Cloudy.
	2	0	42,5	39,5	29,15		NE	1	Cloudy.
22	8	0	36,0	38,0	29,25		NNW	1	Cloudy.
	2	0	40,5	41,5	29,26		WSW	1	Fair.
23	8	0	29,0	36,5	29,20		SSW	1	Fair.
	2	0	34,0	37,0	29,22		SW	1	Fine.
24	8	0	34,0	35,5	29,38		W by S	1	Foggy.
	2	0	37,5	40,5	29,45		WSW	1	Foggy and sun-shine.
25	8	0	36,0	36,5	29,15	0,245	ESE	1	Rainy.
	2	0	40,0	37,0	29,09		ESE	1	Cloudy and rainy.
26	8	0	43,0	38,0	28,75	0,237	W	1	Rainy.
	2	0	41,0	40,5	28,93		W	1	Cloudy.
27	8	0	33,0	38,0	29,03	0,044	ENE	1	Cloudy.
	2	0	59,0	38,0	28,92		NE	2	Rainy.
28	8	0	48,0	43,0	28,84	0,467	SSW	2	Fair.
	2	0	48,0	46,0	28,97		SSW	2	Rainy.
29	8	0	35,0	40,0	28,96	0,580	N by W	1	Rainy.
	2	0	43,0	43,0	29,22		NW	1	Fine.
30	8	0	37,0	42,0	29,20	0,189	W by N	2	Fair.
	2	0	45,0	44,0	29,78		SW	2	Fine.

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for December 1779.

	Time.		Therm. without	Therm. within.	Barom	Rain.	Winds.		Weather.
	H.	M.			Inches	Inch.	Points.	Str.	
Dec. 1	8	0	43,5	43,5	29,45		SW	1	Cloudy.
	2	0	48,0	48,0	29,51		SW	1	Cloudy.
2	8	0	55,5	49,5	29,26	0,236	SW	2	Rainy.
	2	0	58,0	52,0	29,24		SW	2	Rainy and windy.
3	8	0	53,5	55,0	29,29	0,253	W by N	3	Much wind and rain last n.
	2	0	50,0	54,5	29,57		SW	2	Fine.
4	8	0	36,5	46,5	29,65	0,470	SW	2	Fair.
	2	0	42,0	47,0	29,85		SW	2	Fine.
5	8	0	33,0	42,0	30,07		NW	1	Fair.
	2	0	38,5	42,0	30,13		NW	1	Fine.
6	8	0	31,5	38,0	30,27		W by S	1	Foggy.
	2	0	35,0	45,0	30,29		SE	1	Foggy.
7	8	0	40,0	38,0	29,70		SSW	1	Cloudy.
	2	0	45,0	41,0	29,52		SW	1	Rainy and foggy.
8	8	0	40,0	43,0	29,67	0,722	NNE	2	Cloudy.
	2	0	39,0	42,5	29,88		NE	1	Fine.
9	8	0	36,5	38,5	29,85	0,047	SSW	1	Rainy.
	2	0	46,5	41,5	29,66		SW	1	Rainy.
10	8	0	47,0	46,0	29,75	0,439	S by W	1	Cloudy.
	2	0	44,0	47,5	29,75		SW	1	Rainy and cloudy.
11	8	0	47,0	49,0	29,75	0,110	SW	1	Cloudy.
	2	0	48,0	50,0	29,81		NW	1	Fine.
12	8	0	43,0	46,5	29,56	0,030	E	1	Rainy.
	2	0	50,0	48,0	29,45		NW	1	Rainy.
13	8	0	45,5	47,5	29,16	0,125	SSW	2	Cloudy.
	2	0	45,0	48,0	29,06		SSW	2	Rainy.
14	8	0	38,0	44,0	29,41	0,913	W by S	2	Fair.
	2	0	42,5	44,5	29,57		WSW	2	Fine.
15	8	0	34,0	40,0	29,91		S by W	1	Cloudy and foggy.
	2	0	37,0	41,0	29,88		S by W	1	Snow and foggy.
16	8	0	34,5	38,0	29,75	0,193	NE	1	Cloudy and foggy.
	2	0	41,0	40,0	29,64		NE	1	Cloudy and foggy.

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	Time.		Therm. without	Therm. within.	Barom.	Rain.	Winds.		Weather.
	H.	M.			Inches.	Inch.	Points.	Str.	
Dec. 17	8	0	49,0	46,0	29,58	0,140	WSW	1	Foggy.
	2	0	53,0	58,5	29,60		WSW	1	Cloudy and rain.
18	8	0	51,0	50,5	29,62	0,398	SW	1	Cloudy.
	2	0	54,0	52,0	29,65		SW	1	Fine.
19	8	0	50,0	51,0	29,48	0,034	SW	2	Cloudy.
	2	0	52,0	53,0	29,47		SW,	2	Cloudy.
20	8	0	48,0	52,0	28,91	0,201	SSW	2	Fair.
	2	0	50,0	52,5	28,85		SW	2	Fine.
21	8	0	44,0	49,5	29,00	0,179	SSE	1	Cloudy.
	2	0	49,0	50,5	28,91		SSE	1	Fine.
22	8	0	30,0	42,0	29,89	0,268	NNW	1	Fair.
	2	0	31,5	42,0	29,13		NW	1	Fine.
23	8	0	31,0	37,0	29,40		W by S	1	Frosty.
	2	0	34,5	38,0	29,43		SSW	1	Fine.
24	8	0	26,5	35,0	29,63		SSE	1	Foggy and frosty.
	2	0	31,0	35,5	29,64		NNW	1	Fine and frosty.
25	8	0	20,0	31,0	29,82		WNW	1	Frosty.
	2	0	28,5	31,5	29,88		NW	1	Fine and frosty.
26	8	0	20,0	28,0	30,04		W by N	1	Frosty.
	2	0	32,0	30,0	30,07		NW	1	Frosty and foggy.
27	8	0	33,0	32,0	29,98		NNE	1	Cloudy.
	2	0	35,0	33,0	29,91		NE	1	Foggy and snow.
28	8	0	36,0	35,0	29,81	0,095	NE	1	Cloudy.
	2	0	36,0	35,0	29,91		NE	1	Foggy and rain.
29	8	0	36,0	36,5	30,13	0,030	NE	1	Cloudy.
	2	0	38,0	36,0	30,19		NE	1	Cloudy.
30	8	0	35,0	36,0	30,30		ESE	1	Cloudy.
	2	0	47,0	37,0	30,33		SSE	1	Fine.
31	8	0	29,0	35,0	30,24		E	1	Foggy and frosty.
	2	0	34,0	36,0	30,26		E	1	Fine.

METE-

1779	Thermometer without.			Thermometer within.			Barometer.			Rain.
	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Greateſt Height.	Leaſt Height.	Mean Height.	Inches.
January	50,0	25,0	37,2	48,5	31,5	38,2	30,61	29,63	30,24	0,216
February	60,5	37,0	47,9	53,5	44,0	48,8	30,53	29,84	30,26	0,239
March	73,0	33,0	49,2	56,0	35,0	48,1	30,63	29,63	30,19	0,523
April	78,0	38,0	53,8	64,0	47,0	54,8	30,59	29,43	29,96	1,224
May	82,5	41,5	58,4	79,0	45,0	59,4	30,27	29,51	29,89	1,708
June	75,0	52,5	61,7	69,0	51,5	64,0	30,22	29,48	29,92	2,476
July	85,0	57,0	68,0	81,0	63,0	69,5	30,42	29,30	29,91	6,401
Auguſt	84,5	55,0	67,4	79,0	62,0	69,4	30,29	29,64	30,05	1,074
September	79,0	52,0	63,4	78,0	58,0	65,3	30,20	29,54	29,87	2,232
October	64,5	36,0	54,2	68,0	43,5	56,8	30,42	29,54	29,96	3,031
November	69,5	29,0	44,2	65,5	35,5	45,5	30,39	28,75	29,62	2,778
December	58,0	20,0	42,1	58,5	28,0	42,0	30,33	28,85	29,50	4,883
Whole Year			53,9			55,1			29,43	26,785

VARIA-

VARIATION NEEDLE.

Month.	7 h. A.M.	12 h. M	2 h. P.M.	10 or 11 h. P.M.
	° ′	° ′	° ′	° ′
July 2	22 19	22 29	22 38	22 31
3	22 16	22 13	22 35	22 27
4	22 25	22 35	22 21	22 28
5	22 27	22 34	22 38	22 25
6	22 18	22 34	22 37	22 54
7	22 25	22 40	22 38	22 17
8	22 15	22 34	22 25	22 34
9	22 32	22 39	22 49	22 39
10	22 18	22 28	22 16	22 34
11	22 19	22 31	22 34	22 32
12	22 22	22 37	22 41	22 30
13	22 21	22 44	22 30	22 25
14	22 17	22 34	22 00	22 14
15	22 24	22 39	22 41	22 47
Means	22 21	22 34	22 32	22 31
Mean of all 22° 29′½.				

Mean, allowing for the error of the inftrument, *vide* vol. LXVI. p. 382. and vol. LXIX. p. 321. = 22° 28′.

Therefore, as the variation at the Society's houfe feems to be 15′½ greater than it ought to be by reafon of the magnetifm of the building (*vide* fame place) the true variation feems to be 22° 4′½.

 DIP-

DIPPING NEEDLE.

Month.	7 h. A.M.	12 h. M.	2 h. P.M.	10 or 11 h. P.M.	Mean.	
July 2	72° 30′	72° 45′	72° 45′	72° 45′	° ′	
3	72 50	72 45	72 50	72 45		
4	72 50	72 45	72 45	72 50	72 46	West marked end up.
5	72 50	72 50	72 45	72 40		
6	72 50	71 55	71 50	71 55		
7	71 59	71 59	71 50	71 59	71 55	East marked end up.
8	71 59	71 50	72 0	72 10		
9	72 0	72 5	71 50	72 10		
10	72 10	72 10	72 10	72 10	72 7	West marked end down.
11	72 10	72 10	72 15	72 10		
12	72 50	72 40	72 40	72 40		
13	72 30	72 40	72 30	72 35		
14	72 50	72 50	72 10	72 30	72 36	East marked end down.
15	72 30	72 40	72 30	72 40		
					72 21	

THE END OF VOL. LXX. PART I.

APPENDIX.

I. *Translation of a short Extract from a Journal kept by* C. P. Thunberg, *M. D. during his Voyage to, and Residence in, the Empire of* Japan, *in a Letter addressed to the President.* See page 143.

SIR,

DURING my short residence in London, where you did me the honour of introducing me to many men of learning, conversations frequently arose, in which questions were asked of me concerning the empire of Japan: to these I could at that time give answers only from memory; but, having now got possession of my papers, I have drawn out, for the farther satisfaction of the Royal Society, and your particular friends, the following short extract of a journal which I kept regularly during a residence of sixteen months in that distant country.

To you, SIR, it is already known, that I was sent out by the directors of the Botanic Gardens at Amsterdam, and some other eminent men of that place; first to the Cape of Good Hope, and from thence to Japan: in order to investigate the natural history of those countries, and to send from thence seeds and living plants of unknown kinds, for the use of their collections in Holland. At the first of these places I resided three years; and during that time had the good fortune to observe and describe many new species both of animals and vegetables.

In the year 1775 I sailed from thence for Batavia, and after a short stay there, embarked on board a Dutch ship, called Staveniffe, bound for Japan, in company with the Blyenburg. On

 the

the 21ſt of June, we ſailed and paſſed Pulo Sapatoo, the Coaſt of China, and the Iſland Formoſa. On the 13th of Auguſt we made the land of Japan, and the day after were off the harbour of Nagaſacci, the only one in that empire where foreign ſhips are allowed to anchor.

During this paſſage we met with ſevere gales of wind, in one of which the Blyenburg, having received much damage in her maſts, parted company, and (as we afterwards learned) was obliged to go back to Canton, to refit.

We ſailed into the harbour of Nagaſacci with our colours flying, and ſaluted the Papenburg, the Emperor's and Empreſs's guard, and the Town itſelf. During this time there came on board of us two Over Banjoſes, ſeveral interpreters, and inferior officers, and ſome people belonging to the Dutch Factory.

Theſe Over Banjoſes may be compared to the Mandarins of China: a place is prepared for them upon the ſhip's deck, and ſome of them (for they are frequently changed) muſt be preſent when any thing is taken out of, or received into, her. They inſpect every thing, muſter the people, give paſſports to ſuch as go on ſhore, and every day report to the governor of Nagaſacci the proceedings on board.

The attention and care with which theſe gentlemen execute the orders iſſued by the Imperial Court in 1775 is well worthy of relation. The moſt minute articles which are carried out of a ſhip undergo a jealous inſpection, both when they are put into the boats, and when they are landed from them; and the ſame caution is uſed in embarking goods from the ſhore.

Bedding is ripped open, and the very feathers examined. Cheſts are not only emptied of their contents, but the boards of which they are made are ſearched, leſt contraband goods ſhould be concealed in their ſubſtance. Pots of ſweetmeats and of butter are ſtirred round with an iron ſkewer. Our cheeſes had a more narrow inſpection; a large hole was cut into the middle of each, and a knife thruſt into the ſides of it in every direction: even the eggs were not exempted from ſuſpicion; many of them were broken, leſt they ſhould conceal contraband goods within them.

Ourſelves,

Ourſelves, from the higheſt to the loweſt, underwent the ſame ſuſpicious ſcrutiny whenever we went from or returned on board the ſhip. Our backs were firſt ſtroked down by the hand of the inſpector; our ſides, bellies, and thighs, were then in like manner examined; ſo that it was next to impoſſible that any thing could be concealed.

Formerly they were leſs exact in this viſitation; the chief of the factory and captain of the veſſel were even exempted from it. This priviledge they uſed in its utmoſt extent: each dreſſed himſelf in a great coat, in which were two large pockets, or rather ſacks, for the reception of contraband goods, and they generally paſſed backwards and forwards three times a day.

Abuſes of this nature irritated the Japan government ſo much, that they reſolved to make new regulations. For ſome time they found, that the more dexterity they uſed in detecting the tricks of the Europeans, the more dextrouſly they contrived to evade them: at laſt, however, by repeated trials, they have ſo compleatly abridged their liberties, that it is now almoſt, if not abſolutely impoſſible, to ſmuggle any thing.

The complexions of the Japaneſe are in general yellowiſh, although ſome few, generally women, are almoſt white. Their narrow eyes and high eye-brows are like thoſe of the Chineſe and Tartars. Their noſes, though not flat, are ſhorter and thicker than ours. Their hair is univerſally black; and ſuch a ſameneſs of faſhion reigns through this whole empire, that the head-dreſs is the ſame from the emperor to the peaſant.

The mode of the men's head-dreſs is ſingular; the middle part of their heads, from the forehead very far back, is cloſe-ſhaven; the hair remaining round the temples and nape of the neck is turned up and tied upon the top of the head into a kind of bruſh about as long as a finger; this bruſh is again lapped round with white thread, and bent a little backwards.

The women preſerve all their hair, and, drawing it together on the top of the head, roll it round a loop, and faſtening it down with pins, to which ornaments are affixed, draw out the ſides till they appear like little wings; behind this a comb is ſtuck in.

Phyficians and priefts are the only exception to the general fashion; they fhave their heads intirely, and are by that means diftinguifhed from the reft of the people.

The fafhion of their cloaths has alfo remained the fame from the higheft antiquity. They confift of one or more loofe gowns, tied about the middle with a fafh; the women wear them much longer than the men, and dragging on the ground. In fummer they are very thin; but in winter quilted with filk or cotton wadding.

People of rank have them made of filk; the lower clafs of cotton ftuffs. Women generally wear a greater number of them than men, and have them more ornamented, often with gold or filver flowers woven into the ftuff.

Thefe gowns are generally left open at the breaft; their fleeves are very wide, but partly fewed up in front, fo as to make a kind of pocket, into which they can eafily put their hands, and in this they generally carry papers, or fuch like light things.

Men of confequence are diftinguifhed from thofe of inferior rank by a fhort jacket of thin black ftuff, which is worn over their gowns, and trowfers open on the fides, but fewed together near the bottom, which take in their fkirts. Some ufe drawers, but all have their legs naked. They wear fandals of ftraw, faftened to their feet by a bow paffing over the inftep, and a ftring which paffes between the great toe and that next to it, fixing to the bow. In winter they have focks of linen, and in rainy or dirty weather wooden fhoes.

They never cover their heads but on a journey, when they ufe a conical cap made of ftraw; at other times they defend themfelves from the fun or the rain by fans or umbrellas.

In their fafh they faften the fabre, fan, and tobacco-pipe; the fabre always on the left fide, and (contrary to our European cuftom) with the fharp edge uppermoft. Thofe who are in public employments wear two, the one confiderably longer than the other.

Their houfes are built with upright pofts, croffed and wattled with bamboo, plaiftered both without and within, and white-

washed. They generally have two stories; but the uppermost is low, and seldom inhabited. The roofs are covered with pan-tiles, large and heavy, but neatly made. The floors are elevated two feet from the ground, and covered with planks. On these are laid mats which are double, and filled with straw three or four inches thick. The whole house consists of one large room; but may be divided at pleasure into several smaller, by partitions made with frames of wood, filled up with painted paper, that fix into grooves made for that purpose in the floor and cieling. The windows are also frames of wood, divided into squares, filled up with very thin white paper, transparent enough to answer tolerably well the purpose of glass.

They have no furniture in their rooms; neither tables, chairs, stools, benches, cup-boards, or even beds. Their custom is to sit down on their heels upon the mats, which are always soft and clean. Their victuals are served up to them on a low board, raised but a few inches from the floor, and one dish only at a time. Mirrors they have, but never fix them up in their houses as ornamental furniture; they are made of a compound metal, and used only at their toilets.

Notwithstanding the severity of their winters, which oblige them to warm their houses from November to March, they have neither fire-places nor stoves: instead of these they use large copper pots standing upon legs; these are lined on the inside with loam, on which ashes are laid to some depth, and charcoal lighted upon them, which seems to be prepared in some manner which renders the fumes of it not at all dangerous.

The Portuguese, in all probability, first introduced the use of tobacco into Japan: however, be that as it may, they use it now with great frugality, though both sexes, old and young, continually smoke it, blowing out the smoke through their nostrils. The first compliment offered to a stranger in their houses is a dish of tea and a pipe of tobacco. Their pipes have mouth-pieces and bowls of brass or white copper. The hollow of the bowl is so small as scarce to contain an ordinary pea. The tobacco is cut as fine as a hair about a finger's length, and is rolled up in small balls like pills, to fit the small hollow in the bowl

bowl of the pipe; which pills, as they can laſt but for a few whiffs, muſt be very frequently renewed.

Fans are uſed by both ſexes equally, and are, within or without doors, their inſeparable companions.

The whole nation are naturally cleanly; every houſe, whether public or private, has a bath, of which conſtant and daily uſe is made by the whole family.

You ſeldom meet a man who has not his mark imprinted on the ſleeves and back of his cloaths, in the ſame colour in which the pattern is printed: white ſpots are left in manufacturing them, for the purpoſe of inſerting theſe marks.

Obedience to parents and reſpect to ſuperiors is the characteriſtick of this nation. It is pleaſing to ſee the reſpect with which inferiors treat thoſe of high rank: if they meet them abroad, they ſtop till they have paſſed by; if in a houſe, they keep at a diſtance, bowing their heads to the ground. Their ſalutations and converſations between equals abound alſo with civility and politeneſs; to this children are early accuſtomed by the example of their parents.

Their penal laws are very ſevere; but puniſhments are ſeldom inflicted. Perhaps there is no country where fewer crimes againſt ſociety are committed.

Their uſage of names differs from that of all other nations. The family name is never made uſe of but in ſigning ſolemn contracts, and the particular name by which individuals are diſtinguiſhed in converſation varies according to the age or ſituation of the perſon who makes uſe of it; ſo that ſometimes the ſame perſon is, in his life-time, known by five or ſix different names.

They reckon their age by even years, not regarding whether they were born at the beginning or the end of a year, ſo that a child is ſaid to be a year old on the new year's day next after his birth, even though he has not been born many days.

Commerce and manufactures flouriſh here, though, as theſe people have few wants, they are not carried to the extent which we ſee in Europe. Agriculture is ſo well underſtood, that the whole country, even to the tops of the hills, is cultivated. They trade with no foreigners but the Dutch and Chineſe, and in both caſes with

with companies of priviledged merchants. The Dutch export copper and raw camphire, for which they give in return ſugar, ripe cloves, ſappan wood, ivory, tin, lead, tortoiſe-ſhell, chintzs, and a few trifles more.

As the Dutch company do not pay duty in Japan, either on their exports or imports, they ſend an annual preſent to the court, conſiſting of cloth, chintzs, ſuccotas, cottons, ſtuffs, and trinkets.

I had the ſatisfaction to attend the ambaſſador, who was intruſted with theſe preſents, on his journey to Jeddo, the capital of this vaſt empire, ſituated at an immenſe diſtance from Nagaſacci, a journey on which three Europeans only are permitted to go, attended by two hundred Japaneſe at leaſt.

We left our little iſland of Dezima, and the town of Nagaſacci, on the 4th of March, 1776, and travelled through Cocora to Simonoſeki, where we arrived on the 12th, and found a veſſel prepared for us; we embarked on board her, and coaſted along to Fiogo. From thence we travelled by land to Oſacca, one of the principal commercial towns in the empire. At this place we remained the 8th and 9th of April, and on the 10th arrived at Miaco, the reſidence of the Dairi, or eccleſiaſtical emperor. Here we alſo ſtayed two days; but after that made the beſt of our way to Jeddo, where we arrived on the 1ſt of May.

We were carried by men in a kind of palankins, called Norimons, covered, and provided with windows. The preſents alſo and our proviſions were carried on men's ſhoulders, except a few articles, which were loaded on pack-horſes. The Japaneſe officers who attended us provided us with every thing, ſo that our journey was by no means troubleſome.

On the 18th we had an audience of the Cubo, or temporal emperor, of the heir-apparent, and of the twelve ſenators; the day following, of the eccleſiaſtical governors, the governors of the town, and other high officers. On the 23d we had our audience of leave. We left Jeddo on the 26th of May, and arrived at Miaco on the 7th of June. Here we had an audience of the emperor's viceroy, to whom we alſo made preſents, as we were not allowed to ſee the Dairi, or eccleſiaſtical emperor.

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On the 11th we procured leave to walk about the town, and viſit the temples and principal buildings. In the evening we ſet out for Oſacea, which town we were alſo permitted to view, which we did on the 13th.

We ſaw temples, theatres, and many curious buildings; but, above all, the manufactory of copper, which is melted here, and no where elſe in the empire.

On the 14th we had an audience of the governors of this town; after which we reſumed our journey to Fiogo, where we again embarked on the 18th, and proceeded by ſea to Simonoſeki, from whence we arrived on the 23d at Cocota, and from thence were carried in Norimons to Nagaſacci, and arrived at our little iſland Dezima on the laſt day of June, after an abſence of one hundred and eighteen days.

III. *Translation of an Essay written by the Abbé* Fontana, *on the* American *Poison called* Ticunas. See page 163.

THE experiments which I made at Paris during two years on the poison of the viper, and which are the sequel of many others on the same subject, published in Italy ten years before, have enabled me to pronounce with safety on the nature and properties of that poison. The unexpected and important effects which I observed on the application of the poison of that animal to the bodies of living creatures, have led me to new discoveries in animal physics; and these discoveries have gradually led me to doubt of some certain medical theories, either not sufficiently proved, or too generally applied by practitioners.

From that time I have been desirous of extending my researches to other poisons; and, if it had been possible, I could have wished to examine some of the most active vegetable poisons. I had imagined, the animal poisons were like the poison of the viper, which freely diffuses itself through the body of an animal when applied to a wound, but is not increased thereby in the manner in which the poison which produces the small pox, or the canine madness, is augmented: I say, I conceived, that these poisons might have much analogy to one another, and that they might act in the same manner, and upon the same parts of animals. On the other hand, I did not dare to conjecture any thing concerning the operation of vegetable poisons, which I had not yet examined; nor did I think that any thing could safely be advanced concerning the action of them, even after the instructions derived from the best writers on them. Their manner of experimenting was very different from that which I had used in examining the poison of the viper, and their inferences appeared too vague and uncertain. Being arrived at London, however, I had it easily in my power to satisfy my desires on this head. Dr. HEBERDEN, an eminent physician

physician there, and fellow of the Royal Society, procured me a great number of American arrows which had been carefully preserved, and were well impregnated with poison. He was also so obliging as to supply me with a good quantity of poison, inclosed and sealed up in an earthen vessel inclosed in a tin case. Within the tin case was a paper containing the following words: " Indian poison, brought from the banks of the river " of the Amazons, by Don PEDRO MALDONADO: it is one of the " sorts mentioned in the Phil. Transf. vol. XLVII. numb. XII." In the volume of the Transactions here quoted, mention is made of two poisons little different in their activity; the one called the poison of Lamas, and the other of Ticunas. The poison contained in the earthen vessel which I used is that of the Ticunas. It is not well known to which of the two the poison on the arrows belonged; but my experiments proved it was of the same strength with that of the Ticunas, so that I do not think it at all material to distinguish the one from the other.

Much has been written by authors concerning the activity of these American poisons; so that I thought it proper to make my experiments by degrees, and with all possible precaution. The very smell of them was thought to be noxious, on the bare opening of the vessel; and if the least of their particles was suffered to diffuse itself through the air, some grievous disorder, and even death itself, was apprehended; so, at least, we read in the best authors. I began, therefore, as soon as the vessel which contained the poison was open, by making a young pigeon breathe the air of it; for which purpose I held its head within the vessel for a few minutes. On taking it out I found it as well as at first. I loosened with a pen-knife many pieces of the poison, in order to have a little dust in the vessel, and then again immersed the head of the pigeon; but I found that in this second experiment also the animal suffered nothing.

From that moment I made no more difficulty of exposing myself to that vapour, and of smelling the effluvia, which seemed disagreeable and nauseous. Many of the particles entered my mouth with the air, and I found that they had a taste something

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like liquorice. The ſmell, therefore, of this poiſon, when dry, is perfectly innocent; as are alſo the particles which enter with the air into the mouth and noſe, and thence go to the lungs.

But the caſe in which it ſeems that this poiſon is moſt dreaded (although its application be ſtill external) is, when it is reduced to vapour or ſmoke by burning on the coals; or when, after boiling a conſiderable time, it riſes in denſe fumes. I wiſhed to try it in both theſe ways, and therefore threw many pieces of the dry poiſon upon burning coals, and cauſed the pigeon to breathe the fumes, by holding its head in the middle of them; but it never ſhewed any ſigns of ſuffering any pain. I went ſtill farther: I took a glaſs tube ſix inches long, and four wide, and filled it full of this denſe and white fume, and then introduced the ſame pigeon into it; but it ſhewed no more ſigns of ſuffering than if it had been held in the fumes of burning ſugar. I afterwards boiled a good quantity of it in an earthen veſſel. I expoſed the pigeon to the vapours both when the poiſon began to have ſome conſiſtence, and when, by more boiling, it began to burn to the ſides of the veſſel, and to be reduced into very denſe vapours, and to a coal; but ſtill the animal ſuffered nothing from theſe trials. I then made no ſcruple of freely ſmelling it and expoſing myſelf to the fumes of it. The odour of the dry poiſon, when burnt on the coals, is very diſguſting, and ſmells like burnt excrement.

From all theſe experiments I draw this concluſion, to wit, that the vapours or fumes of the American poiſon, when ſmelled or breathed, are innocent. Mr. DE LA CONDAMINE was certainly deceived when he wrote that this poiſon is prepared by women condemned to die; and that it is known to be come to its perfection when the vapours, which it emits in boiling, kill the perſon who is obliged to be preſent.

This poiſon diſſolves eaſily and very well in water, even when it is cold, and ſo it does alſo in the mineral and vegetable acids. But it diſſolves in oil of vitriol much more ſlowly than in the other acids, and becomes as black as ink by the operation, which it does not do with any of the other acids. It does not effervesce with either acids or alkalies; neither does it alter milk, nor tinge it

except with its natural colour; nor does it tinge the vegetable juices, either red or green. When examined with the microscope, there is no appearance of regularity or of crystallization; but it is for the most part made up of very small irregular roundish bodies, like vegetable juices. It dries without making any noise, in which it differs from the poison of the viper, and it has an extremely bitter taste when put upon the tongue.

From all which I deduce, that it is neither an acid nor an alkali, nor composed of salts that are visible even with the microscope.

It was not so much through curiosity as on account of the order which I had prescribed to myself in making my experiments, that I was led to examine if this poison is fatal to life, when applied immediately to the eyes, or if it excites any disease or irritation of the parts. I had before found, that the poison of the viper was as innocent when put upon the eyes as it is in the mouth and in the stomach; whence I was curious to see the analogy between these two poisons, both so active, and yet of such different origins.

I began, therefore, by putting a small quantity of it, dissolved in water, on the eye of a Guinea-pig. The animal shewed no signs of suffering, neither at the time nor afterwards, nor was the eye in the least inflamed. Two hours after, I repeated the experiment on both the eyes of the same animal, and with a greater quantity of the poison; but the pig did not feel the least inconvenience, and the eyes remained in the natural state. I tried the experiments on the eyes of two other Guinea-pigs with the same success, which constantly attended all the experiments which I afterwards made on the eyes of many other animals, and especially on those of rabbits. I could never observe that it made any alteration in their eyes any more than if I had bathed them with pure water: from whence, I think, it may be concluded, that the American poison is not in the least hurtful when applied to the eyes, and that it exerts no extraordinary action upon them.

But will it be innocent when taken in by the mouth and swallowed? Mr. DE LA CONDAMINE, and all others who have treated

treated of this poiſon, believe it to be quite innocent when received by the mouth; and this is the common opinion of all the Americans. The reaſon they give for it is, that they can eat with impunity the animals killed with this poiſon, or rather with the poiſoned arrows. This reaſon is more ſpecious than convincing; ſince it may be a poiſon when introduced into the blood, even in the ſmalleſt quantity, and yet not be ſuch when taken in by the mouth, except the doſe be very conſiderably increaſed.

The following are the experiments which I have made, the reſult of which is to render us cautious of pronouncing, even after we have conſulted experience itſelf.

I made a young rabbit ſwallow two grains of the poiſon, diſſolved in water, and then forced it to drink a tea-ſpoonful of water, to waſh all the poiſon out of its mouth into its ſtomach. This animal ſhewed no ſigns of ſuffering either then or afterwards.

In like manner I made another young rabbit drink three grains of the poiſon, and it ſuffered nothing, any more than the former had done. I made another young rabbit drink four grains of the poiſon, and it likewiſe ſuffered nothing. I repeated the ſame experiments on three other young rabbits, to the laſt of which I gave ſix grains of the poiſon, but ſtill without any effect.

I then concluded that theſe experiments were ſufficient to aſſure me, that the American poiſon is innocent when taken by the mouth, as the poiſon of the viper is; but I was deceived. I had the curioſity to try it on a young pigeon, to which I gave ſix grains to drink, and it died in leſs than twenty minutes. I repeated the experiment on two other pigeons, and they both died within the half hour.

Theſe laſt experiments being contradictory to the former, obliged me to try ſeveral over again on the rabbits and on Guinea-pigs. I gave, therefore, to a ſmall Guinea-pig five grains of the poiſon to drink, and I found it dead after twenty-five minutes. I then made a young rabbit drink eight grains of the poiſon; at the end of the half hour it did not ſeem affected; but

but in half an hour more it tottered; four minutes after it fell down as if it were dead, and in four minutes more it was dead. I made two other young rabbits, and two other ſmall Guinea-pigs, drink nine grains of the poiſon: the two pigs died in twenty minutes, and one of the rabbits died in leſs than forty-five minutes. Theſe experiments induced me to believe, that a greater doſe of the poiſon may prove ſtill more certain death; and that the ſame quantity of poiſon produced different effects in the ſame animals, according to the ſtate their ſtomachs happened to be in at the time. I had generally obſerved, in making the experiments, that after ſwallowing the poiſon, thoſe animals which had their ſtomachs pretty full of meat either did not ſuffer any thing, or elſe died with much difficulty. I was deſirous of making this clearer, by experiments on three rabbits and two pigeons, which I therefore firſt kept for a long time without meat. Three grains of poiſon only killed each of them in leſs than thirty-five minutes. I repeated the experiment on five other animals with full ſtomachs, and only one of them died.

From hence I deduce this certain fact, that the American poiſon, when taken in by the mouth, is a poiſon; but that it requires a pretty large quantity of it to kill even a ſmall animal. The facts above related concerning the American poiſon, which is noxious when taken in large doſes, make me think, that the poiſon of the viper, although it is innocent when taken by the mouth in a ſmall quantity, may be mortal when taken in a greater quantity. That torpor which it excites on the tongue, and which continues ſo long, is enough to convince us, that it is not quite inactive, and that it may really be fatal when taken in a large quantity. I intend to try this experiment on ſome future occaſion, when I propoſe to give the collected poiſon of eighteen or twenty vipers to a ſmall animal when its ſtomach is empty, and I dare venture to prophecy, that it will die; for ſince a very ſmall doſe can take away motion and ſenſation from the tongue, or, in other words, deprive that organ of its principles of life, a greater quantity ought to deſtroy thoſe of the organs more eſſential to life itſelf. If we conſider that poiſon taken

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in by the mouth muſt extend itſelf over a very large ſurface which is always moiſt, and mix itſelf with the food in the ſtomach, and that the abſorbing veſſels are extremely ſmall, it will no longer ſeem ſtrange, that it is not noxious when taken in a ſmall quantity, which we have juſt ſeen to be the caſe with the American poiſon.

I began my experiments on the activity of this poiſon by wounding different parts of animals with a lancet wetted in the poiſon diſſolved in water. I wounded a ſmall Guinea-pig with it in the thigh three times at different intervals. The lancet was full of poiſon, yet the animal ſuffered no harm. I made the ſame trial on three other little pigs and a rabbit, but none of them either died or ſuffered any injury. In all theſe caſes the blood flowed evidently from the wounds: from which I ſuſpected that the poiſon could not diffuſe itſelf, but that it was driven back, as I had obſerved in the caſe of the poiſon of the viper, which, for this reaſon, is frequently harmleſs.

My ſuſpicion was ſoon confirmed by the following experiments. I ſoaked a ſingle thread in the poiſon, and paſſed it through the ſkin of a Guinea-pig near one of the nipples, but yet no diſorder followed. I then ſoaked another thread thrice doubled, and let it firſt dry a little, for fear the poiſon ſhould remain behind on the ſkin, in drawing the thread through it. I paſſed it through the ſkin of the thighs of a ſmall rabbit near the belly: in ſix minutes the rabbit began to ſhake and ſhew ſigns of weakneſs. In another minute it fell down motionleſs, appeared convulſed at intervals, and was quite dead in ſix minutes more.

I repeated this ſame experiment, of the ſoaked double thread, on two other rabbits, and on three Guinea-pigs; all of which fell down, and were convulſed in ſix or ſeven minutes, and died within the half hour.

I had the curioſity to try if the American poiſon could communicate itſelf to animals, and kill them, when applied to the ſkin barely ſcratched, or ſcarcely wounded with the point of a lancet.

I had obſerved at Paris, that the poiſon of the viper communicated a local diſorder in ſuch caſes, and that it affected and

disordered the ſkin, but did not prove fatal. The American poiſon, on the contrary, never produces any local diſeaſe, as I obſerved in making the experiments related above, but leaves the wounded parts as it found them. This conſtitutes an eſſential difference between theſe two poiſons.

I clipped off the hair, with a pair of ſciſſars, from a part of the thigh of a ſmall Guinea-pig, and ſcratched the ſkin lightly with a file. There was no viſible diſcharge of blood; but certain ſmall red ſpots and a moiſtneſs appeared on the ſkin. I bathed the part with a little drop of the poiſon diſſolved in water. In ten minutes the animal gave ſigns of convulſions; a little after it fell down motionleſs, except convulſions, which it had now-and-then more or leſs ſtrong, and it died in twenty minutes. The part of the ſkin where the poiſon had been applied was not at all altered. This experiment was attended with the ſame ſucceſs on two other Guinea-pigs, and on a ſmall rabbit; all the three dying in leſs than twenty-ſeven minutes with very evident ſigns of convulſions. I wiſhed to try if the larger animals could reſiſt this poiſon, when only applied to the ſcratched ſkin; and therefore, with the point of a lancet, I wounded, very lightly, in many places, the ſkin of a large rabbit, having firſt cleared the part of the hair, and then I bathed the wounded places with ſeveral drops of the poiſon. After fifteen minutes it became leſs briſk than before, and ſhook its head now-and-then, as if it could not hold it up without difficulty; but in twenty minutes more it became as lively as at firſt. I repeated the experiment on another ſomewhat ſmaller rabbit: in ten minutes it gave the ſame kind of ſhakes with its head, and could hardly go or ſupport itſelf on its feet; but after other twenty minutes it was as lively as ever.

I ſhaved off about an inch of the ſkin of a pretty large rabbit; a little blood appeared although the fleſh did not ſeem to be cut; I put about three drops of the poiſon on the place: in ſix minutes the rabbit ſeemed very faint: after another minute it fell down as if dead; it ſcarce breathed, and was at times convulſed; but in leſs than forty-ſix minutes it recovered the uſe of its

its feet, and a little after began to eat, and remained without any more signs of being disordered.

I scratched the skin on the thigh of a hen, and applied the poison to it; but it continued well, although I repeated the experiments twice on other parts of the skin.

I slightly scarified the skin of a pigeon's thigh, and applied to it the poison dissolved in water. After twenty-five minutes it was so weak that it could not stand, and was convulsed at intervals. It fell down a little after, as if it were dead, and remained in that state of apparent death above three hours. By degrees, however, it began to recover, and in half an hour more it was quite well.

This experiment on pigeons was repeated five times. Three of them died in less than twenty minutes; and the other two were seized with convulsions, but afterwards they recovered.

From other experiments made afterwards, both on birds and beasts, it may be concluded, that the American poison applied to the skin slightly scratched may be fatal, although it is not so always, nor in all circumstances. The larger animals more easily resist the action of the poison; and when the more feeble animals did not die, in a little time they were as well as ever.

I was desirous to know what quantity of the poison was necessary to kill an animal. I had made a like inquiry concerning the poison of the viper, and had determined the quantity of that poison requisite to kill the different animals. I might, indeed, have safely concluded, that a very small quantity of the American poison is sufficient to kill a small animal, since one or two small drops applied to the skin just scratched, had proved fatal to more than one; but I wished for something more positive.

I steeped a very small bit of cotton into about one-fiftieth of a little drop of poison dissolved, which could scarcely be the one-fiftieth part of the whole drop. This I introduced into a muscle of the thigh of a pigeon, but the animal was not affected by it.

Two hours after, I put into another muscle an atom of the dry poison, scarcely perceptible to the eyes: this likewise did not affect the pigeon. I repeated the experiment with the dry poison on three other pigeons, but none of them either died or

was ſick, though in one of the caſes the bit of dry poiſon was very perceptible. I made the ſame experiment on three Guinea-pigs, and on two ſmall rabbits, ſtill with the ſame ſucceſs, none of them being at all affected. It muſt be obſerved, the poiſon was not diſſolved by the humours of the wound, and I found the bits of it quite intire.

I put to the muſcle of another pigeon a bit of cotton much larger than before, impregnated with about eight times as much poiſon as in the former caſe: in ſix minutes time the pigeon fell down, and died ſoon after. To the muſcles of two Guinea-pigs I applied bits of cotton ſteeped in much the ſame quantity of poiſon as above: one of them died in twelve minutes, and the other fell down, as if dead, in ſix minutes, but it recovered a little time after.

From theſe experiments I conclude, that above one-hundredth part of a grain is requiſite to kill a ſmall animal; and that it is neceſſary that the poiſon be diſſolved, for it to prove fatal, or to cauſe any alteration in the animal oeconomy.

I have made various experiments to determine whether the American poiſon be fatal or hurtful when applied to the wounded combs of poultry, or to the ears of quadrupeds ſlightly wounded. The poiſon of the viper is not commonly fatal in theſe parts, nor is there any viſible diſorder in the poiſoned comb, though there is in the wattles, which ſwell horribly, ſo as ſometimes to kill the animal.

I wounded the comb of a fowl in many parts, and twice applied to it the American poiſon by means of cotton well ſoaked in it, but without being able to produce any diſorder. But the experiment ſucceeded better when tried on the ears. After having made many fruitleſs trials to communicate the poiſon by ſcratching or wounding the ears of many rabbits, all which ſhewed no ſigns of injury; I at laſt ſucceeded in killing two in leſs than thirty minutes by the application of a great quantity of the poiſon to the more fleſhy parts of the ears, which I had wounded in many places with the point of the lancet.

The experiments on the ears have evinced to me, that where there are few blood veſſels, either no diſorder is produced, or

elſe it is not mortal. In this reſpect the American poiſon has much analogy to that of the viper. As the poiſon of the viper is quite innocent when applied to the tendons and ligaments, eſpecially if they are without blood veſſels, ſo the American poiſon is equally innocent when applied to the ſame parts. It is ſuperfluous to relate the ſequel of theſe experiments.

I was deſirous of knowing whether the American poiſon were more ſurely fatal when introduced into the muſcles than when applied to the ſkin, though drawn through the latter from ſide to ſide. A large Guinea-pig, which two days before had twice undergone the operation of the ſkin cut, without ſuffering any diſorder, and a third time with but little ſigns of being affected, died in leſs than twelve minutes after I had applied the poiſon to the wounded fibres of a muſcle of its thigh. It fell down motionleſs after the firſt three minutes. I repeated the experiment ten times on Guinea-pigs, pigeons, and middle-ſized rabbits, and all the animals died; ſo that there can be no doubt but that poiſoned wounds in the muſcles are more fatal than thoſe in the ſkin, or in the ears, or in the combs of fowls. The more certain method, however, of ſucceeding is, to ſoak well a piece of very porous wood, cut very ſharp, in the poiſon, and ſo introduce it into the ſubſtance of the muſcle laid bare for that purpoſe. But even this method failed three times that I tried it on the combs of fowls: nor did I ever obſerve any appearance of diſorder, although the wood was well ſoaked, and although I left it for ſeveral hours in the combs.

On this occaſion I made uſe of the arrows; many of which I employed in perforating the ſkin of animals, and many others in piercing the muſcles. All the animals, eſpecially the larger rabbits, which were wounded in the ſkin, did not die, though the greater part of them did; but none of thoſe recovered which were pierced in the muſcles. In general, I found that the arrows are more dangerous, and oftener fatal, than the poiſon diſſolved in water, and then ſimply applied to the wounded parts. I found the poiſon on the arrows more active after ſteeping

them in warm water, as they then operated both more ſpeedily and more ſurely; and their activity is ſtill more increaſed by ſoaking them in the poiſon, boiled in water to the conſiſtence of julep. Various large animals, ſuch as rabbits, have fallen down motionleſs in this manner in leſs than two minutes; and ſome of the ſmaller ſort have been viſibly affected in leſs than one minute.

I introduced one of the arrows, that had been well ſoaked in the boiled poiſon, into the comb of a fowl, and left it there a whole day, without any appearance of injury to the animal. The next day I perforated the comb and the wattles with two arrows prepared as before, and left them there for ten hours; but ſtill without any effect. I then perforated one of the muſcles of the thigh with an arrow, and the animal died in forty-two minutes.

I had propoſed to myſelf, amongſt other things, to examine what alteration this poiſon may undergo by uniting it with acids and with alkalies. This I had tried with the poiſon of the viper, the noxious qualities of which neither the moſt powerful mineral acids, nor the moſt active alkalies, could take away. For this purpoſe then I diſſolved the poiſon in three mineral acids, as alſo in diſtilled vinegar, and in rum; and about an hour after I made the following experiments.

I made ſome ſmall gaſhes in the ſkin of a ſmall Guinea-pig, and covered it over ſeveral times with the poiſon diſſolved in the nitrous acid: the animal appeared to ſuffer nothing but the mechanical inconvenience of the wound and the acid; within an hour after it was as lively as ever. Two hours after I repeated the experiment on another part of the ſkin prepared as above, which I covered with the poiſon diſſolved in rum; the animal died in leſs than four minutes.

I ſlightly wounded the ſkin of a young rabbit, and applied to it many drops of the poiſon diſſolved in oil of vitriol: it ſeemed to ſuffer nothing, and was as lively as before. Four hours after I prepared another part of the ſkin as above, and applied to it a few drops of the poiſon diſſolved in diſtilled vinegar: the animal fell down after four minutes, and was quite dead in ſix.

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I prepared, likewiſe, the ſkin of another ſmall rabbit, and covered it with the poiſon diſſolved in the marine acid; but the animal was not affected. Six hours after I applied the poiſon, diſſolved in rum, to another part of the ſkin, and in forty-five minutes it fell, and was convulſed; but it recovered in leſs than an hour.

From theſe firſt experiments it ſhould ſeem, that mineral acids render the poiſon innocent; and that, on the contrary, vinegar and rum make no alteration in it. I continued my experiments with the poiſon diſſolved in vinegar and in rum, but the reſults were ſomewhat various. Of ſix animals, to which was given the poiſon diſſolved in vinegar, only two died; two others were very ſick, and the remaining two were not at all affected. Of ſix others, treated in like manner, with the poiſon diſſolved in rum, five died; and the ſixth was very ſick. From hence it appears, that the poiſon diſſolved in theſe two fluids preſerves its noxious quality.

On the other hand, I repeated the experiments with the poiſon diſſolved in mineral acids on ſix animals; and not one of them was in the leaſt affected. I began to ſuſpect, that the poiſon might fail in its effect, not becauſe it had loſt its noxious quality, but becauſe it could not inſinuate itſelf into the wounded parts, on account of the too great action of the mineral acids on the ſkin and veſſels, which they ſhrivel and burn up. To clear up this doubt, I evaporated by heat the poiſon diſſolved in the mineral acids, and, when it was quite dry, applied it many times to ſeveral animals in various parts of their ſkin; but none of them were hurt by it.

It appears, therefore, that the mineral acid deſtroys the noxious quality of the American poiſon: I only ſay, *it appears*, becauſe it may, perhaps, be ſuſpected, that, as there remained a little of the acid mixed with the poiſon after evaporation, it was this that produced the uſual alteration in the veſſels of the ſkin. I ought to have made ſome other experiments with it waſhed ſeveral times in water, and rendered quite inſipid; but at that time I was in want of animals to aſcertain the truth of this ſuſpicion, and I have not had time ſince to reſume the ſubject.

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With refpect to the alkaline falts, I may fay, that I have not difcovered that they alter the poifon, or render it in the leaft lefs noxious. It is true, indeed, that I have not fo often repeated the experiments, nor fo much varied them, as I ought to, and as I fhould have done, if I had not found a great difficulty in procuring animals, and if I had not had in view other experiments much more important.

It was natural for me to think, that as the acids hinder the action of the poifon on animals, they might alfo be a remedy againft that poifon.

I prepared, therefore, in the ufual way, the fkin of a Guinea-pig, and covered it with the poifon; and about forty feconds after I wafhed it with the nitrous acid, and afterwards with pure water: the animal fuffered nothing. Two hours after I introduced the poifon into a mufcle, and immediately applied the nitrous acid to the part; but the animal fell in a moment convulfed, and was quite dead in two minutes.

I repeated this experiment on the mufcles of another Guinea-pig, and as foon as I had applied the poifon, wafhed it with the nitrous acid, and a little after with water. It fell convulfed in two minutes, and was quite dead in four.

I poifoned, in like manner, the mufcles of four pigeons, and the moment after I wafhed them with the nitrous acid: they died in one minute. Sufpecting that it might be owing to the nitrous acid rather than to the poifon, I next made ufe of nitrous acid, much diluted, on four other pigeons; but they all four died, although much more flowly. Being defirous of knowing whether the fimple application of the nitrous acid to the mufcles could kill pigeons and fmall Guinea-pigs, I made the trial on two of each fort: the pigeons both died foon after; but neither of the pigs, although one of them was very fick.

It appears, therefore, that acids are not only ufelefs as a remedy; but fometimes dangerous, when applied to the poifoned mufcles of animals.

I fhall not fay any thing of any other remedy which I have tried, becaufe I have found by experience that they are all ufelefs, whether they are applied foon or late, or whether externally

nally or internally. When the poison is introduced deeply, and has already insinuated itself into the humours, every remedy comes too late and is useless.

There yet remained to be made a further very nice inquiry, and which might in some cases too have turned out a very useful one. My experiments on the poison of the viper led me to make an observation of the same sort on the American poison. I had determined from them the time which the poison of the viper employs in diffusing itself into the body of the animals; and when it might be useful to cut away the poisoned part, or to make a ligature about it, to hinder the poison from communicating itself to the animal by the blood.

I introduced into the muscle of a pigeon's leg an American arrow steeped in warm water, and left it there; and four minutes after I made a ligature moderately tight, immediately above the wound. In twenty-six hours the animal seemed to suffer no inconvenience but from the ligature. I then withdrew the arrow, and loosed the ligature. The part was a little swelled and livid; but the animal did not die of it, though it could make no use of its leg for many days, and afterwards used it with some difficulty.

I pierced the muscles of another pigeon with a fresh arrow, as above, and applied the ligature six minutes afterwards, leaving the arrow in the wound. In four minutes the pigeon had not strength to hold up its head, or scarcely to stand; presently after it fell down to all appearance dead, and was quite dead in six minutes more.

I repeated the experiment on another pigeon, leaving the arrow in the muscles again, and eight minutes after I tied the bandage about the leg. Three minutes after it was visibly sick, but recovered again a little afterwards. It was still living twenty-six hours after, although the muscles were livid. I then loosed the bandage, and it died in two hours.

I subjected a fourth pigeon to the same experiments, leaving the arrow in the muscles, and applying the bandage five minutes after: it died in two hours.

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I performed the ſame experiments on four other pigeons, to all of which I applied the bandage in two minutes. Ten hours after they were all living; I then took off the bandage, after which three of them died, and the fourth recovered perfectly.

I again repeated the ſame experiments in like manner on other four pigeons, but left the bandage on thirty hours: only one of them died, and that was not till two days after, and certainly from the effect of the bandage being too tight, which had produced a gangrene in the muſcles. Theſe ſame experiments I have alſo repeated on much younger pigeons, whoſe legs may be cut off below their thighs without their dying. None of thoſe died whoſe legs were cut off two minutes after they were poiſoned; and only two out of ten died of thoſe whoſe legs were cut off after three minutes. Fewer pigeons died by this method than by the bandage, when they were both applied after equal times. The reaſon of this is, that the amputation is not attended with death or any remarkable diſorder in the animals, whereas the bandage often produces the gangrene in the parts wounded by the arrows, and the pigeons frequently die of the gangrene. I made alſo the ſame experiments on ſome ſmall Guinea-pigs, and on many young rabbits, ſometimes cutting off the part, and ſometimes applying the bandage. The reſults were in general ſimilar to thoſe obſerved in the pigeons, although not quite ſo regular or certain.

In general I found, that it required a certain time for the American poiſon to communicate itſelf to animals, that this time is much greater than that which the poiſon of the viper requires to communicate itſelf, and that the effects of the American poiſon on animals are more vague and more various; and that, finally, animals may be cured of the effects of both theſe poiſons by cutting away the part in time, when it can be done without endangering the life of the animal by the operation itſelf.

In the courſe of my experiments on the poiſon of the viper, I found that it is not poiſonous to all animals; and that there are ſome cold-blooded animals to whom it is quite innocent. I was curious

curious to know if the case was the same with the American poison. All the writers on the American poison tell us, that it is poisonous to all animals; but assertions are very different from facts. Many experiments are necessary to evince this general conclusion, and it does not appear that enough have been made to warrant it.

I began by impregnating the muscles of frogs with the poison, and they died in a little time. I then had recourse to eels, into which I introduced the arrows, towards the lower or tail parts of them; and they all died, though very slowly.

I had found that the poison of the viper is quite innocent to the viper itself, and to those serpents which in Tuscany are called *binchi*, and by the French, *couleuvres.* Of these last I could procure no more than two; for which reason I could make but few experiments on them; but those I have made I think quite decisive. I wounded one of them towards the tail with an arrow well covered with poison of the thickness of syrup, and left the arrow in the muscles. I had previously made an incision in the place where I introduced the arrow, that the dissolved poison upon the arrow might likewise easily enter the muscles with the arrow. I also applied some more poison to the same part by means of small incisions in the muscles. The serpent did not seem affected by the poison; but for many hours was as well as ever. I locked it up in a box, which having opened six hours after, I found that the serpent was gone, nor could I ever find it again.

I repeated the experiment many times, at different intervals, on another rather smaller. The last time I introduced two poisoned arrows into the muscles of the tail, and left them there for twenty-four hours. I frequently applied the poison thickened to a syrup to the wounds, and introduced a great quantity into them with a tooth-pick, yet, so far from dying, the animal was not sensibly hurt.

I have often made this same experiment on vipers, without any one of them dying by the poison, although some were wounded in the muscles towards the tail with many arrows well impregnated with poison thickened to a syrup. I have

left the arrows for twenty or thirty hours together in the mufcles, and yet none of them have died. It is true, indeed, that fome few, after being operated on, appeared lefs lively than before; and it feemed, that the wounded parts, or the lower half of the body, had fenfibly loft fome of its natural motion, and this torpor continued in fome for many hours; but then again others continued as lively as before.

After all thefe experiments it may be fafely afferted, that the American poifon is intirely innoxious to cold-blooded animals, as is the poifon of the viper; in which refpect thefe two poifons have a great analogy, although the one be no more than an animal gum, as I have fhewn elfewhere, and the other a mere vegetable juice.

It remained to examine the action of this poifon on living animals; and alfo which are the particular parts of animals that are affected by it when it proves fatal.

Every thing tended to make one think, that it excites one of thofe diforders which modern phyficians call nervous. The fymptoms are precifely and decifively the fymptoms of thofe difeafes. Convulfions, faintnefs, a total lofs of ftrength and motion, a diminution or intire want of fenfation, are the ordinary fymptoms of the poifon, in animals. It has often been obferved, that very lively animals become in a moment fenfelefs and motionlefs, and feem at the point of death. I have generally obferved a fymptom which feems to be a real demonftration that the diforder produced by this poifon is purely nervous. If the animals do not die in a few minutes, they perfectly recover again, although they have been thrown into a ftate of lethargy often for hours, and have not given any certain and evident figns of life. Now this is the very cafe with the diforders which are called nervous. They frequently come on at once; they fometimes excite convulfive motions, and fometimes they deprive the patient of all ftrength; but as foon as the effects of the diforder ceafe, the patient becomes perfectly well, and is hardly fenfible that he has been ill. Notwithftanding all this, thefe figns could not impofe on me after my experiments on the poifon of the viper: for the diforder produced by that poifon

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has alſo ſome ſymptoms of the nervous diſorders, and it appears that the nerves are chiefly affected, although experiments have determined the contrary: therefore, we ought here alſo to have recourſe to experiments, and not ſuffer ourſelves to be ſeduced by an unfounded theory, and by ſpecious reaſonings.

To proceed methodically in this important queſtion, I thought it would be proper to begin by examining whether the American poiſon produces any ſenſible alteration in the blood drawn from the veins of animals, when it is mixed with it.

I cut off the head of a pigeon, and received its warm blood into two warm conical glaſſes, to the amount of about eighty drops into each. Into the one glaſs I put four drops of water, and into the other four drops of the poiſon, diſſolved in water as uſual; the quantity of poiſon in theſe four drops ſcarcely amounting to one grain in weight when dry. I ſtirred round equally the contents of the two glaſſes for a few ſeconds, in order to mix the materials well together. In two minutes the blood which was mixed with the pure water was coagulated; but the other blood which was united with the American poiſon never coagulated, but became darker and blacker than the former, which remained red as uſual. Three hours after it was ſtill as fluid as at firſt, while in the other glaſs the ſerum appeared to be already ſeparated from the red part.

I examined the blood of both the glaſſes with a microſcope, both at that time and afterwards, and found that the red globules ſtill preſerved their original figure, and that there was no difference between the two in this reſpect.

I repeated this experiment many times with the ſame ſucceſs; ſo that it is evident, that the American poiſon does not ſenſibly alter the red globules of the blood in the circumſtance above mentioned. It is, however, worthy of obſervation, that this poiſon is ſo far from coagulating the blood, that it abſolutely prevents the coagulation which happens in the blood after it is drawn from animals; yet it cannot be ſaid to attenuate or diſſolve the blood, ſince nothing of that kind is obſerved when it is examined with a microſcope, the red part remaining figured as in its

natural ſtate, and nothing more ſubtle or more thin being obſervable in that fluid.

A circumſtance perfectly ſimilar I alſo obſerved to happen with the poiſon of the viper; ſo that the effects or alterations cauſed by theſe two poiſons in the blood drawn from the veſſels, appear to be perfectly ſimilar, both of them hindering the blood from coagulating, yet neither of them diſſolving or altering the globules of it: the only difference between them is, that the poiſon of the viper tinges the blood much blacker than the American poiſon does.

The poiſon of the viper does not alter the globules of the blood even when it is given to the living animal, and that the animal in conſequence dies. I have obſerved the ſame thing with reſpect to thoſe animals which are killed by the American poiſon; ſo that the two poiſons agree in a wonderful manner in all theſe caſes. But as the poiſon of the viper produces in general a ſenſible alteration in the maſs of the blood of thoſe animals that are bit by it; I thought that the ſame attention ought to be paid to the examination of the blood of thoſe animals which have died of the American poiſon.

I have obſerved in general, that the muſcles of animals killed by the American poiſon were paler than before; the blood veſſels near the heart appeared more turgid than uſual; the blood a little darker coloured than ordinary, though not much, nor coagulated; the viſcera of the abdomen not ſenſibly affected; the heart and the auricles in the natural ſtate, although the heart had ſometimes its external veſſels more viſible, and that they appeared as if they were injected.

But I have obſerved a great alteration in one of the viſcera, the moſt eſſential to life, to wit, the lungs, which always appeared greatly affected. I have generally found them ſpotted more or leſs; often with large and livid ſpots, and ſometimes they ſeemed to be quite putrified. This effect on a viſcus ſo eſſential to life deſerves the greateſt attention: it appeared to me, that it was the greater the longer the animal had lived after being poiſoned. I have obſerved the lungs of ſome animals to

be here and there tranſparent, eſpecially towards the edges. The pulmonary air was very viſible through the external membrane: I have examined it with a microſcope, and have been able to diſtinguiſh very well the ſmall pulmonary bladders, ſtreaked with veſſels for the moſt part without blood.

Great as this alteration is in ſo important a viſcus, I could not perſuade myſelf, that it could alone produce ſo great and inſtantaneous a diſorder, and that the whole force of the poiſon exerted itſelf only upon the blood and lungs. There was, indeed, the inſtance of the poiſon of the viper, which produces ſomething ſimilar; but then this poiſon produces a kind of general coagulum in the blood itſelf, which certainly is not obſervable from the American poiſon.

In an inquiry ſo important, and at the ſame time ſo obſcure, I thought I ought to have recourſe to experiment itſelf, and to examine the effects of the American poiſon when introduced immediately into the blood. For this purpoſe I made uſe of the ſame method which I had before employed for introducing the poiſon of the viper into the blood of the jugular. I uſed a turn-cock of glaſs, bent at the point, inſtead of a ſmall ſyringe. With this cock I took up the American poiſon diſſolved in water, and then opening the jugular vein thruſt the point into it. As the method of making this ſort of experiments has been before deſcribed in my treatiſe on the poiſon of the viper, I think it unneceſſary to give a particular deſcription of it here. The experiment is ſo conducted, that the poiſon enters the blood by the jugular without touching any cut part of the veſſels, not even thoſe of the jugular itſelf.

For the firſt experiment I put into the glaſs ſyringe four drops of the poiſon diſſolved in water; the quantity of poiſon in the four drops could hardly amount to half a grain. I introduced the crooked end of the ſyringe into the jugular of a large rabbit; but in the act of puſhing in the piſton, which was not cloſe enough fitted to the bore of the ſyringe, the poiſon returned backward, which made me ſay to the perſons who were preſent, that the experiment had failed; but I was ſurprized when they anſwered me, that the animal was already dead. I believe there

were

were not ten ſeconds between the moment in which I ſaw the poiſon turned back, and that of my being told that the animal was dead, as in fact it was. I cannot ſay what quantity of poiſon was introduced into the blood; there muſt have entered a ſufficient quantity, as the animal was killed; but had it not been for that circumſtance, I ſhould have judged, from the quantity returned back, that none at all had entered the jugular. The animal was ſo dead, that there appeared no ſigns of reſpiration, and the whole body was more pendent and flaccid in all its parts than is uſual with animals that have been long dead. The death of this animal followed ſo cloſe upon the introduction of the poiſon, that the interval of time ſeemed quite inſenſible: it appeared to take place much quicker than in the caſe of the poiſon of the viper, introduced into the blood in the ſame manner.

Having repaired my ſyringe, I put into it only two drops of the water in which I had mixed one quarter of a drop of the poiſon diſſolved in water as above. I had ſcarcely introduced the poiſon into the jugular, when the rabbit fell down as dead as if it had been ſtruck with lightning. I do not believe that half a-drop of the liquor in the ſyringe was introduced when the animal fell motionleſs and dead.

In general, from other experiments which I made afterwards, I think I may venture to ſay, that this poiſon, introduced immediately into the blood by the jugular, kills ſooner, and requires a leſs quantity of it to kill than the poiſon of the viper does. Death follows the introduction of the poiſon into the blood ſo ſpeedily, that it prevents the uſual convulſions of the animal. If a ſmaller quantity of the poiſon be taken, we then perceive the ordinary convulſions and palpitations, and death does not happen ſo ſuddenly.

It is indeed true, that the blood is not coagulated, nor ſo much altered in its colour, as when the poiſon of the viper is introduced into the jugular; but death is not, therefore, more tardy, nor leſs certain, as both the poiſons, when introduced immediately into the blood, kill animals in the ſame manner.

This

This then is a truth gained from experiment, to which nothing can be objected, however dark or little underſtood the cauſe of death may be in ſuch caſes: viz. that the American poiſon, introduced into the blood, kills the animal inſtantaneouſly; from whence alſo it indubitably appears, that when it is externally applied to a wounded part of a living animal, it can and ought to communicate, by means of the blood, a great diſorder into the animal oeconomy, and occaſion death itſelf. The death of the animal, which follows the inſtant that the poiſon is introduced into the blood by the jugular, proves to a demonſtration, that in ſuch caſes all the action of the poiſon is againſt the blood itſelf, and that the nervous ſyſtem is not at all affected or altered. This, however, is ſtill no proof that the nerves may not be more or leſs affected by that poiſon, when death happens much ſlower, and when it is applied externally to wounded parts. In this caſe we perceive the convulſions, and all the ſigns of a nervous diſorder. The nerves may, therefore, probably be affected by the poiſon, and be the chief cauſe of the death of the animal.

We muſt here, however, ſtill have recourſe to direct experiment, as was done with reſpect to the poiſon of the viper, and ſee what diſorders and diſeaſes are produced by the American poiſon when applied immediately to the nerves without touching the veſſels.

My experiments have been made on the ſciatic nerves of large rabbits, which I prepared in the ſame manner as I had done at Paris, when I was making experiments with the poiſon of the viper, ſo that I ſhall not here give any particulars concerning the method of preparing theſe nerves. I ſhall mention, however, a ſmall number of the principal experiments made on the nerves, by which may be ſeen the variety of ſucceſſes I met with, eſpecially in the firſt trials, which would have deceived me if I had not perſiſted in multiplying my experiments, and varied them in proportion as I found leſs agreement in the reſults. To this perſeverance, or obſtinacy as I may call it, I chiefly owe the new diſcoveries which, I believe, I have made concerning the two poiſons of the viper and the Ticunas.

Having

Having laid bare the ſciatic nerve of a rabbit, I paſſed under it a fine rag ſeveral times doubled, and put upon the nerve a little cotton well ſoaked in the American poiſon, thickened to a ſyrup. I covered the nerve with the ſame rag, that the poiſon might not run over the opened muſcles of the animal, and ſewed up the ſkin as uſual. After ten minutes the rabbit began to have convulſions, and to totter; it then fell with all the ſigns accompanying the effects of the poiſon, and died ſoon after.

I repeated the ſame experiment on another rabbit, and took care to wrap up the poiſoned nerve with rags ſtill better than before. This ſecond rabbit ſhewed no ſigns of being affected for ten hours, during which I obſerved it; but looking at it after two hours more, I found it had been dead a little while, as it was ſtill warm.

I ſuſpected that the poiſon applied to the nerve, which was conſiderable in quantity, might at length have penetrated through the rags, and uniting with the humours of the parts cut, have extended its action to the muſcles and the adjacent parts. I was under the neceſſity, therefore, of either diminiſhing the quantity of poiſon, or increaſing the rags, to prevent any diffuſion of the poiſon through them: I adopted the latter as the more ſecure way.

I detached the ſciatic nerve of a rabbit as uſual, and introduced below it a very fine rag, often doubled. I put the bit of cotton, well ſoaked in the poiſon, on the nerve, and covered every thing well with the lappets of the rag. This rabbit lived twenty-four hours, and only ſhewed ſigns of being ill in the laſt hour, nor was there any reaſon to think that it died of the effects of the poiſon.

I prepared the ſciatic nerve of another rabbit in the ſame manner, covering it with the poiſon and rags as before. This rabbit died forty hours after, without any ſymptoms of being poiſoned.

I made the ſame experiment on the ſciatic nerve of three other rabbits, having taken all poſſible care that the poiſoned nerve ſhould be well covered with rags, that there might not be any reaſon to ſuſpect the poiſon might penetrate through them.

One

One of them died three days after, but the other two were ſtill living at the end of eight days.

I alſo prepared the nerves of two other rabbits exactly as above, but without the poiſon, to ſerve as a kind of comparative experiment to the former. One of theſe rabbits died in thirty-ſix hours, and the other was ſtill living eight days after.

Theſe experiments ſeemed ſufficient ground to determine, whether the American poiſon, when applied externally to the nerves, is capable of producing any malady or diſorder in animals. But it remained to be tried, whether it be equally inactive when applied to wounded nerves, as alſo to the pulp of the nerves.

I prepared the ſciatic nerve of a rabbit as above, and I pierced it ſeveral times through with a lancet, before I applied the poiſon to it, and then put the poiſon exactly upon the wounded part of the nerve. The rabbit lived five days, and then died without any apparent illneſs. I repeated the experiment on another rabbit with the ſame circumſtances, and it was ſtill living eight days after.

I varied the experiment a little on the nerves of three other rabbits. Inſtead of wounding the nerve in many places, I made an inciſion, above five-twelfths of an inch long, into it, and introduced into the ſlit ſome threads, well ſoaked in the poiſon, and covered the whole up very well. One of theſe died in ſixty hours, but ſeemingly not from the poiſon; the other two were living eight days after.

I thought it neceſſary to vary alſo this ſecond kind of experiment, and to cut the nerve as I had done in examining the poiſon of the viper. In conſequence I cut the ſciatic nerve as far as I could from the top, to be able eaſily to wrap the rags about it. The detached part of the nerve in the largeſt rabbits might be about an inch and a half. Having placed the nerve on the rags I covered it well with poiſon in the part where it was cut, and covered the whole up with the rags as uſual.

I performed this experiment on ſix rabbits. Two of them died in forty hours; two others in three days; but the remaining two were living four days after.

For a comparative experiment I prepared, as above, the nerves of two other rabbits, which I cut, but did not apply the poiſon to: one of them died in thirty-ſix hours, but the other was living the third day after.

The uniformity of the reſults of theſe experiments upon the nerves induced me to think it quite ſuperfluous to repeat them; and, I am perſuaded, they will leave no doubt with any one who is accuſtomed to experiments, and not prejudiced in favour of ill grounded hypotheſes. Hence it follows, that the American poiſon is not poiſonous when applied to the nerves; and that in ſuch caſes it produces no ſenſible diſorder in the oeconomy of living animals: this is what the experiment directly eſtabliſhes. But to ſuppoſe what has not been obſerved; to believe what is contradicted by experiments is dreaming in philoſophy, running after error inſtead of truth, and adopting mere fancies for facts. The American poiſon (ſimilar in this reſpect to the poiſon of the viper) is not poiſonous, but quite innoxious, applied in any manner whatſoever, to the nerves. But it kills in a moment, and with the ſmalleſt quantity, when introduced immediately into the blood by the jugular, as does likewiſe the poiſon of the viper. Its action is therefore all upon the blood, and not in the leaſt upon the nerves, whatever may be the principle or the mechaniſm by which death is produced.

The effects and alterations cauſed in the blood by the poiſon of the viper are more determinate and more evident. Here a coagulation undeniably happens, which is not obſervable in the blood of animals killed by the American poiſon; but the latter produces a great change in the lungs, which are greatly diſordered by it.

It is true, that death happens ſo ſuddenly after introducing the American poiſon into the veſſels, that one cannot well comprehend how it can take place in ſo ſhort a time; for it may be ſaid, that the poiſon is hardly arrived at the heart before the animal is dead: nor is it well underſtood, how cold-blooded animals can be killed by it (frogs, for inſtance, which live ſo long with the circulation ſtopped) although it be true that they die much ſlower by theſe poiſons than other animals whoſe blood is

is warm. A humour, or the blood's being affected by a poiſon, may by degrees produce, in cold-blooded animals, diſorders ſtill greater than thoſe which may be cauſed by ſtopping the circulation.

By death taking place immediately on introducing the poiſon into the blood, we may be induced to ſuſpect, that there exiſts in that fluid a very active, ſubtle, and volatile principle, which eludes the acuteſt ſight, and even the microſcope itſelf. This principle may on this hypotheſis appear neceſſary to life, and againſt this principle the poiſon may be ſuppoſed chiefly to direct its operation.

That there really exiſts in the blood a very active and volatile principle, may very well be ſuſpected from ſeeing that the poiſon of the viper prevents the coagulation of the blood when it is drawn from the veſſels, whilſt, on the contrary, it produces a coagulation of it within the veſſels. It may be ſuppoſed, that ſomething evaporates from the blood when it is drawn out, which exiſts in it while within the veſſels. On this hypotheſis this active and vital principle may be conſidered as the reſult of the whole animal oeconomy, not excluding the nerves which may moſtly contribute to it. But all this is only mere conjecture, more or leſs probable, and unſupported by experiment. We ought to abide by ſure facts, let the mode of explaining them be what it may. Now theſe facts are, that the American poiſon does not act on the nerves, and that its action is intirely on the blood.

Before my experiments no perſon would have doubted but that the action of the American poiſon was immediately on the nerves. All the external ſigns declared it to be ſo. Theſe ſigns then are equivocal, and they are falſely adopted by phyſicians for the certain proofs that a diſeaſe is purely nervous. All theſe ſymptoms may exiſt without the nerves being in the leaſt affected: the alteration of the blood alone is ſufficient to produce them in a moment. The principal phyſicians have attributed the diſeaſe produced by the poiſon of the viper, and by the American poiſon, to an alteration in the nerves; it belongs to them now to examine, whether other diſeaſes, ſuppoſed generally to

be nervous, be not rather diſeaſes of the fluids, than diſeaſes of the blood. The ſuſpicion is great, the ſigns equivocal; the principle is ſhewn not to be general. I would not here aſſert, that no diſorder could ever be derived from the nerves; this would be running into one extreme in order to avoid another. There is no doubt but that many diſeaſes are nervous in their origin, and that many others become ſo from diſorders which have began in other parts, and thoſe merely fluid. The illneſſes which ariſe from mental uneaſineſs ſhew us the power of the nerves on living bodies. But all this does not prove that all the diſeaſes attributed to the nerves are nervous; and that the ordinary ſigns of this diſorder are not equivocal. And it is certain, that the poiſons we have examined have no immediate action on the nerves, as has been commonly believed hitherto. Some may object to this, that the poiſon of the viper and the American poiſon may act on the extremities of the nerves, and that for this reaſon they are innocent when applied to the trunk of the nerves. But this would be to object merely for the ſake of objection, and to fancy unneceſſary difficulties. The ſmalleſt variation of circumſtance would then be ſufficient grounds for objection; and who may not find a variety in things when it is ſo difficult to meet with two things alike? As for myſelf, I obſerve that the internal ſubſtance of the trunks of the nerves does not appear different from that which forms the extremities of thoſe nerves, that the trunk is ſubject to pain the ſame as the extremities, and that I am no inventor of hypotheſes which are not confirmed by facts.

In the univerſality of the conſequences which I have deduced from my experiments, I may be deceived; and I may even be deceived in ſome of the experiments themſelves, notwithſtanding that they have been very carefully conducted, and that I have ſought after truth without prejudice. I do not doubt, but that thoſe who may apply themſelves to ſuch reſearches after me, may find ſome things to add, and, perhaps, ſome to correct alſo. It is ſufficient for me to have opened a channel to new truths, and that the principal facts which I have advanced are true.

The

The greateſt part of theſe experiments were made in the preſence of Dr. INGENHOUSZ, phyſician to the Emperor, my particular friend; a man who has manifeſted in ſeveral works his poſſeſſing the talents of a true obſerver. Mr. TIBERIUS CAVALLO was alſo preſent at many of the more important ones. I thought that the authority of two gentlemen, ſo well known to the learned, would procure the more credit to my experiments.

After having finiſhed my experiments on the American poiſon, a friend of mine in London procured me a great number of Eaſt Indian arrows. I wiſhed to make ſome experiments on them alſo; but thoſe I have made are neither many nor ſufficiently varied. It appears to me, however, that this poiſon differs not from the other, except in its being leſs active, in killing of animals: which leſſer activity is probably to be attributed either to theſe arrows having been leſs carefully preſerved than thoſe from the Weſt Indies, as really appeared to be the caſe, or elſe to the poiſons having been prepared many years ſince.

I have never ſucceeded in killing any rabbit (even the ſmalleſt-ſized one) with it, by applying it to the ſkin ſcratched or ſlightly wounded, although I have uſed it in greater quantities, and on more extenſive parts of the ſkin, than the poiſon of Ticunas; even in rabbits of ſcarcely a pound weight it did not produce any ſenſible alteration.

I pierced the ſkin of ſeveral animals with the arrows, and let them remain in it ſeveral whole days, without being able to perceive that the animal was affected with the poiſon; but when I perforated the muſcles with the arrows, and left them there, its effects were very obſervable. Several animals died in this manner, and that with all the viſible ſigns of the poiſon, and with all the ſigns or ſymptoms with which animals die who are killed by the American poiſon: it is true, however, that none of them died, or were ſenſibly affected, till after ſeveral hours; ſo that this poiſon ſeems not to differ eſſentially from the other. It perfectly agrees with it when examined with the microſcope, when mixed with turnſol, when thrown into the eyes of animals, when taſted with the tongue, and when chewed between the teeth;

teeth; on the other hand, it does not diffolve fo well in water as the other poifon, for indeed a great part of it remains infoluble in that fluid. The only confequence which can be deduced from the facts above mentioned is, that the poifon is much more noxious when applied to the mufcles than when applied to the fkin, in which refpect alfo it agrees very well with the other poifons. This ftill more convinces us, that the immediate action of thefe poifons is not againft the nerves, fince it is certain, that the fkin is more fenfible than the mufcles, and that it is all interfected with nerves.

I have alfo made a few experiments on the oil of tobacco, the refults of which I thought it might not be improper briefly to relate in this place.

Experiments made with oil of tobacco.

I made a fmall incifion in the right thigh of a pigeon, and applied to it one drop of the oil of tobacco, and in two minutes it loft the ufe of the right foot.

I repeated the fame experiment on another pigeon, with exactly the fame effect.

I made a flight wound in the mufcles of the breaft of a pigeon, and applied the oil of tobacco to it, and in three minutes the animal could not ftand on its left foot.

And this fame experiment was repeated on another pigeon, with the fame fuccefs.

Into the mufcles of the breaft of a pigeon I introduced a toothpick fteeped in the oil of tobacco, and a few feconds after the pigeon fell down feemingly dead.

Having applied the oil of tobacco to two other pigeons, they threw up feveral times all the food they had eaten.

Two others, treated in the fame manner, but with empty ftomachs, made many efforts to vomit.

In general, I found the vomiting to be a conftant effect of this poifon; but the lofs of motion in the part to which the poifon is applied is only accidental.

None

None of the animals, however, died by the application of the oil of tobacco.

Experiments with the water of the Lauro-Ceraſus.

I ſhall conclude my experiments on poiſons with an account of ſome that I have made on a poiſon which has for ſome years become remarkable in Europe. This poiſon is the water of the Lauro-Ceraſus, which is not inferior to any of the other very active poiſons, if conſidered with reſpect to the great diſorder which it introduces into the animal oeconomy, and the ſhort time which it takes to act when given to animals by the mouth. It not only produces the ſtrongeſt convulſions, and death itſelf, even in animals of a middle ſize; but, moreover, when it is given in ſmall doſes, the animal writhes ſo that the head joins the tail, and the vertebræ arch out in ſuch a manner as to fill every body who ſees it with horror. The convulſions and motions of the whole body are extremely violent, and the ſtruggles kill the animal in a ſhort time. If it be given to the animal as a gliſter, it equally produces the convulſions and death. With only two tea ſpoonfuls of the water given by the mouth, I have ſeen middle-ſized rabbits fall down convulſed in thirty ſeconds, and die within a minute. When it is given in great quantities to animals, they die almoſt in an inſtant, and without convulſions, the parts of the body being relaxed and pendent. When it is given in ſmall quantities, the convulſions are more or leſs ſtrong; the hind feet firſt loſe their motion, and afterwards the fore, which die ſlower. When the animal can no longer move its feet, or the reſt of the body, it ſtill moves very well the neck and the head, which it continues to hold up ſtrongly, and to turn round every way. In this ſtate the animal hears ſounds, and ſees objects; and although it has ceaſed to move its feet ſpontaneouſly, yet it ſtill moves them backwards and forwards when they are much pricked or ſqueezed; a ſign it can ſtill move them, though it moves them only on account of the great pain.

The water of the Lauro-Cerafus is, therefore, a ftrong poifon when given by the mouth, or introduced into the body in the manner of a glifter. Its action is fo violent, and fo quick, that it may be faid to begin to act the moment an animal takes it into its mouth; certain it is, that it has fcarce entered the ftomach by the throat before the animal fuffers. It is however true, that very fmall dofes have no effect, that is, a little drop given to a fmall animal produces no fenfible diforder, although the fame quantity of the poifon of Ticunas would be fatal. But that makes no effential difference between this poifon and the other well known poifons.

I have found, that a certain quantity of water put on the leaves of the Lauro-Cerafus produces a liquor which is quite innocent, unlefs the leaves be in a great, and the water in a very fmall quantity. When the water is put feveral times on the fame leaves, and drawn off fucceffively, its activity becomes greater indeed, but ftill is not fufficient to take away life; but if, inftead of a bare mixture with water, the infufion be diftilled in balneo mariæ, the diftilled liquor becomes then a moft powerful poifon, and proves fatal in a very fhort time. I have chiefly ufed it in this way, but I have no doubt but that it might be raifed to fuch a degree of activity, as to prove mortal even when given in as fmall dofes as the American poifon. It might be fufficient to diftil the liquor firft obtained feveral times over again with new leaves well chopped and dried. I believe it would at laft be obtained in the form of a concrete oily fubftance, which, evaporated by fire, would not only be equal in force to any known poifon, but far exceed them all. But I referve this experiment for fome other occafion, when I fhall alfo fpeak of the bitter almonds, and of the degree of poifon to which their water can be raifed by diftilling it till it be dry.

The water of the Lauro-Cerafus kills animals when introduced into the cavity of the body; but what effect does it produce when applied to wounds? It may fuffice here to relate one only of the various experiments which I have made. I opened the fkin of the lower belly of a pretty large rabbit, and made a wound in it of about an inch long, and having flightly wounded the

the muscles under it in many places, I applied to the part two or three tea-spoons full of the water: in less than three minutes the animal fell down convulsed, and died soon after. This experiment shews us, that the water of the Lauro-Cerasus is a poison similar to the others, and that it operates when insinuated into the body by means of wounds made in it. This experiment has been attended with similar results in other warm-blooded animals; but I have always found, that the water of the Lauro-Cerasus, when given by the mouth, acts more powerfully and quicker than in the other way, even when the quantity given is smaller; a circumstance, in my opinion, that deserves the greatest attention, since it is matter of fact, that a large wound offers many more vessels to absorb that poison immediately than the mouth and stomach; besides that the nervous parts ought to be more affected from the very state in which they are put by the wound. It is not the warm-blooded animals alone which are suddenly killed by this water when they are made to drink it, but the cold-blooded animals also die of the effects of it; and what appears to me very singular is, that they die in an extremely short time, and perhaps more quickly than the others; which is quite contrary to what happens from the other poisons. It may suffice for the present to mention eels, which are very difficult animals to kill, and still continue to move their parts when dead. These animals die in a few seconds after having drunk the water, and have scarcely swallowed it when they begin to contract themselves; but death suddenly seizes them, and renders them immoveable in a moment, without leaving even the motion of the parts they usually have on being handled. The heart, indeed, continues to beat, although faintly, but it ceases to move much sooner than when they are killed by cutting off the head. Here it cannot be denied but that the muscular irritability is extremely affected, and in a particular manner. I know not if there be any cold-blooded animal that resists this poison. Those which I tried it on all died; and I doubt whether there be any to which it is not fatal: if so, it deserves a particular distinction, on account of its being the most terrible of all known poisons, as well as for its universality in proving

fatal to all ſorts of animals. But how is it always mortal in ſo ſhort a time, when taken by the mouth, and admitted into the ſtomach, where we do not ſuppoſe there are any veſſels capable of receiving it? This difficulty requires ſome farther experiments. We ought to examine what effects it produces when applied immediately to the nerves, and when introduced into the blood without touching the parts which are cut.

For this purpoſe, I made uſe of large rabbits, and performed the experiments on their ſciatic nerves, in the ſame manner as I had done with the poiſon of the viper and the American poiſon. I ſhall here tranſcribe only one experiment, omitting the reſt for the ſake of brevity, not thinking them neceſſary after the great number which I have already related on the nerves.

Having detached the ſciatic nerve of a large rabbit for above an inch and a half, I introduced under it a wrapper of very fine linen, ſixteen times doubled, that the parts below it might not be penetrated by the water of the Lauro-Ceraſus. I then wounded the nerve with many ſtrokes of the lancet in the longitudinal direction, and covered all this wounded part, which extended above eight lines in length, with a roll of cotton of three lines in thickneſs, well ſteeped in the laurel water. More than fifteen drops of the water were wanted to moiſten the cotton, and this water communicated itſelf directly by the wounds to the medullary ſubſtance of the ſciatic nerve. I covered the whole about a minute after with new rags, ſo that it was impoſſible for the Laurel water to communicate with the parts below it or near it. Having ſewed up the external ſkin, and left the animal at liberty, it ſeemed not to be in the leaſt affected, neither then nor afterwards. It ran about, and eat, and was as lively as ever. In ſhort, the animal was not ſenſibly affected in this way by this poiſon, which kills ſo quickly when it is taken in by the mouth. This caſe, as well as many others, is very ſimilar to thoſe of the poiſon of the viper and of the American poiſon; and it ſhews, that the water of the Lauro-Ceraſus applied immediately upon the nerves, and ſo inſinuated into the medullary ſubſtance of them, is not at all poiſonous; conſequently, that it does not act upon the nerves, however applied externally.

After ſo many experiments) as have been related in the courſe of this eſſay) on the poiſon of the viper, and on the American poiſon, a ſtill more powerful one than the former; and after having ſeen that neither of theſe two poiſons act on the nerves, when applied immediately to them, while they inſtantly kill very ſtrong animals when introduced into the blood; nothing was more natural than to conclude, that the poiſon of the Lauro-Ceraſus, which is equally innocent with the others when applied to the nerves, would alſo prove mortal when introduced into the blood: experience, however, ſhews quite the contrary; ſo true is it that we ought to miſtruſt analogy, even when it appears moſt uniform. I introduced the water of the Lauro-Ceraſus into the jugulars of a large rabbit, to the amount of five drops or upwards, in the ſame manner as I had done the poiſon of the viper and the American poiſon, yet the animal ſhewed no ſigns of ſuffering. I ſuſpected I had not performed the operation right; that I had not introduced any of the water into the veſſels; and that the ſyringe had inſinuated itſelf into the cellular membrane. I therefore repeated the experiment, and again introduced into the jugular a quantity of poiſon, perhaps three or four times greater, and I was very careful to make the point of the ſyringe enter the jugular properly before I introduced the poiſon, that the poiſon ſhould not by any means turn back again; yet ſtill the animal was not affected by it, but continued as lively as ever. I was more ſurprized than ſatisfied with all this. I could not perſuade myſelf, that the water of the Lauro-Ceraſus was not a poiſon, and a very powerful one too, when introduced into the blood, ſince it was poiſonous when applied to wounds in the fleſh, and taken by the mouth, and likewiſe inactive and harmleſs when applied to the nerves. I therefore again repeated the experiment, and this time I introduced by the jugular a whole tea-ſpoonful of the Laurel water; and yet ſtill the animal was not affected. I alſo tried the ſame experiment on another rabbit, into the jugulars of which I introduced a large tea-ſpoonful of the ſame poiſon; yet neither did this rabbit ſhew any ſigns of ſuffering, either then or afterwards.

The unexpected reſult of theſe experiments threw me into a very great uncertainty concerning the action of this poiſon: I could neither comprehend the mode of its operation, nor even upon what parts it acted when taken by the mouth or applied to wounds. Here all was confuſion: it was found neither to act on the nerves nor on the blood, and yet it killed, and that in an inſtant, when introduced into the ſtomach by the mouth.

Is there then a new way of deſtroying the life of animals different from that of the blood and of the nerves? The loſs of motion, and that too in a few ſeconds, in ſuch animals as eels, which in other caſes continue to move for hours after the head is cut off, and even after they are cut in pieces, would make one believe, that the irritability of the muſcular fibres was affected by this poiſon. It is true, indeed, that the heart continues to move in thoſe animals; but that motion is very much leſſened, and laſts but for a very ſhort time. In the warm-blooded animals, juſt killed by this poiſon, there ſtill exiſts ſome motion, but it is very little; and although their heart continues to beat for ſome time, it beats much leſs than when they are killed by other means. The irritability is certainly diminiſhed very much in many animals, and in many others intirely deſtroyed; by whatever means the poiſon kills in ſo ſhort a time, and however obſcure the mechaniſm may be by which the muſcular fibres loſe their irritability. We muſt confeſs our ignorance in our reſearches into nature. When we think we have accompliſhed every thing, we ſuddenly find ourſelves juſt where we began. Experiment is the only guide which we have to conduct us in our reſearches: experiment is indeed a ſecure way of avoiding error, but experiment does not always leads us to the more remote truths, nor always guide us to the knowledge of the ſecret arcana of nature, nor yet always conduct us whither we have propoſed to go.

But although we know not how the Laurel water operates, or, more properly ſpeaking, on what part that poiſon exerts its action, when it kills animals, we know, however, that it is quite innocent when applied immediately to the nerves, and even to their medullary ſubſtance: and it is equally true, that all the experiments above related clearly ſhew, that the poiſon of the viper

viper and the American poiſon are both harmleſs any how applied to the nerves; but that they are both poiſonous when introduced into the blood. Theſe are facts which were before unknown; they are truths now laid open; nor can they be brought into doubt again by any one. Theſe facts deſtroy all the ſyſtems that have been invented by the writers upon the action of thoſe poiſons; and from theſe facts we ought to ſet out to arrive at the knowledge of thoſe poiſons, and of the manner in which they operate.

Some light might probably have been thrown upon the action of the poiſon of the Lauro-Ceraſus, by applying it to different parts of the brain of living animals; but I reſerve this experiment for a more convenient occaſion than the preſent, and till I ſhall have reduced the Laurel water to the conſiſtence of ſyrup. In that ſtate, the poiſon being rendered much more active, will probably offer new and more important facts, and may perhaps give a clearer inſight into its operation; as alſo enable us to judge upon what parts of living animals it acts ſo as to kill them. I ſhall alſo reſerve for that time the trial of whether that poiſon acts upon the lymphatic veſſels, or, to ſpeak more properly, on the lymph itſelf. This is a mere ſuſpicion which I have lately taken up, and which my preſent circumſtances do not permit me to examine at preſent. I am therefore forced to give my experiments on this ſubject, in ſome meaſure, defective and unconnected.

PHILOSOPHICAL

TRANSACTIONS.

PART II.

PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXX. For the Year 1780.

PART II.

Caldwall sc.

LONDON,

SOLD BY LOCKYER DAVIS, AND PETER ELMSLY,
PRINTERS TO THE ROYAL SOCIETY.

MDCCLXXXI.

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OF

VOL. LXX. PART II.

PHILOSOPHICAL

TRANSACTIONS.

XVII. *Theorems for computing Logarithms. By the Rev.* John Hellins; *communicated by the Rev.* Nevil Maſkelyne, *D. D. F. R. S. and Aſtronomer Royal.*

Read March 16, 1780.

THE utility of logarithms is ſo well known, that much need not be ſaid upon it. In our days he muſt be a ſlender mathematician who does not know that they are uſeful, not only in trigonometry, navigation, aſtronomy, the calculations of compound intereſt

and annuities, but alſo in the finding of fluents, and the ſummation of infinite ſeries.

Some of the greateſt mathematicians that this kingdom ever produced, as Sir ISAAC NEWTON, Dr. HALLEY, Mr. COTES, and Mr. SIMPSON, have thought it not beneath them to improve the conſtruction of logarithms, which ſtrongly argues the utility of thoſe artificial numbers, and may ſuggeſt to us that the conſtruction of them cannot be much further improved.

Now, although we ſhould be very diffident in our expectations of improvement in any part of the mathematics after it has been handled by ſuch great men, yet, if the method of computing be in general long and tedious, or if there ſtill remain any particular difficulty, I believe, no good reaſon can be given why every attempt to abridge the one, or remove the other, ſhould be diſcouraged. The eaſy method of computing the logarithms of large numbers given in page 49. of Mr. SIMPSON's pamphlet on Trigonometry is a proof that thoſe gentlemen, who were of opinion that nothing better was to be hoped for, or expected, than what they publiſhed on the ſubject in the beginning of this century, were miſtaken. And the following theorems, inferior to none as to convergency, and uſeful in deducing the logarithms of great fractions from thoſe of ſmall ones, or the

the logarithms of ſmall numbers from thoſe of great ones, may be conſidered as another proof of the miſtake before mentioned. I have only to add here, that theſe theorems are new to me; and if they are ſo to the public, I humbly preſume they will be acceptable.

THEOREM I.

The log. of $\frac{p+q}{p} = 2 \times$ log. of $\frac{2p+2q}{2p+q}$ + log. of $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$.

DEMONSTRATION.

$\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq} \times \frac{\overline{2p+2q}|^2}{\overline{2p+q}|^2} = \frac{1}{\overline{2p+q}|^2-qq} \times \frac{\overline{2p+2q}|^2}{1} = \frac{4pp+8pq+4qq}{4pp+4pq} =$ $\frac{pp+2pq+qq}{p\times\overline{p+q}} = \frac{p+q}{p}$: conſequently, log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq} + 2$ l. $\frac{2p+2q}{2p+q}$ $=$ log. $\frac{p+q}{p}$. Q. E. D.

COROLLARY.

If $q=1$, and we write n for p, the theorem becomes log. $\frac{n+1}{n} = 2$ log. $\frac{2n+2}{2n+1}$ + log. $\frac{\overline{2n+1}|^2}{\overline{2n+1}|^2-1}$, which expreſſion perhaps is of more frequent uſe than that above.

THEOREM II.

Log. $\frac{p+q}{p} = 2$ log. $\frac{2p+q}{2p}$ − log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$.

DEMONSTRATION.

$\frac{2p+2q}{2p+q} \times \frac{2p+q}{2p} = \frac{2p+2q}{2p} = \frac{p+q}{p}$: therefore, log. $\frac{p+q}{p} =$ log. $\frac{2p+2q}{2p+q}$ + log. $\frac{2p+q}{2p}$. But it has been proved above, that log. $\frac{p+q}{p} = 2$ log. $\frac{2p+2q}{2p+q}$ + log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$. If now we take this equation from twice the laſt there will remain 2 log. $\frac{p+q}{p}$ − log. $\frac{p+q}{p} = 2$ log. $\frac{2p+2q}{2p+q}$ + 2 log. $\frac{2p+q}{2p}$ − 2 log. $\frac{2p+2q}{2p+q}$ − log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$: that is, log. $\frac{p+q}{p} = 2$ log. $\frac{2p+q}{2p}$ − log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$. Q. E. D.

COROLLARY.

Putting $q=1$, and $n=p$, as above, we have

$$\text{log. } \frac{n+1}{n} = 2 \text{ log. } \frac{2n+1}{2n} - \text{log. } \frac{\overline{2n+1}|^2}{\overline{2n+1}|^2-1}.$$

I ſhall now ſet down ſome examples of the uſe of theſe theorems beginning with theorem 1.

The firſt example of the utility of this theorem may be in computing the logarithm of the number 2.

It is well known to mathematicians, that the computation of this logarithm was formerly a very laborious

I task:

taſk: and although the work be much ſhortened by help of the converging ſeries invented by the illuſtrious Sir ISAAC NEWTON, ſtill the logarithm of 2 has not been directly computed without many figures by any theorem I have yet ſeen. The eaſieſt computation of it that has come to my hands is in page 44. of the late ingenious Mr. THOMAS SIMPSON's pamphlet on Trigonometry and logarithms. His ſeries conſiſts of the powers of $\frac{1}{3}$.

If now we put $n = 1$ in the theorem ($\log. \frac{n+1}{n} = 2 \log. \frac{2n+2}{2n+1} + \log. \frac{\overline{2n+1}|^2}{\overline{2n+1}|^2 - 1}$) we ſhall have $\log. \frac{2}{1} = 2 \log. \frac{4}{3} + \log. \frac{9}{8}$. Here then the fractions, whoſe odd powers are to be uſed, are $\frac{1}{7}$ and $\frac{1}{17}$; conſequently, in the ſeries formed from $\frac{1}{7}$, about one half of the number of terms taken by Mr. SIMPSON will give the reſult true to as many places of figures as his; and, from the fraction $\frac{1}{17}$, much fewer terms will ſuffice. To ſhow how faſt theſe ſeries converge I will ſet down of each terms, enough to give the logarithm of 2 true to ten places of figures.

The

The odd powers of $\frac{1}{7}$ divided by their respective indices.

1st,	0·14285714286
3d,	0·00097181730
5th,	0·00001189980
7th,	0·00000017347
9th,	0·00000000275
11th,	0·00000000004
The sum,	0·14384103622 is $\frac{1}{2}$ l. of $\frac{4}{3}$.
	4
	0·5753641488 twice l. $\frac{4}{3}$.

The odd powers of $\frac{1}{17}$ divided by their respective indices.

1st,	0·05882352941
3d,	0·00006784721
5th,	0·00000014086
7th,	0·00000000035
The sum,	0·05889151783 is $\frac{1}{2}$ l. of $\frac{9}{8}$.
	2
Log. $\frac{9}{8}$,	0·11778303566
2 log. $\frac{4}{3}$,	0·57536414488
Log. of 2.	0·69314718054

But it is obvious, that this operation gives not only the logarithm of 2 but that of 3 alſo: for the logarithm of 4 being given from that of 2, and the logarithm of $\frac{4}{3}$ computed above, the logarithm of 3 is had, being = log. of 4 − log. of $\frac{4}{3}$.

Log. of 4	1·38629436108
Log. of $\frac{4}{3}$	0·28768207244
Log. of 3	1·09861228864

Other examples of the uſe of theſe theorems in ſhewing how eaſily the logarithms of great fractions are derived from thoſe of ſmall ones.

If the logarithm of $\frac{64}{63}$ were given, or computed, we may very eaſily find the logarithm of $\frac{32}{31}$: for (by theorem

orem 1.) $2 \log. \frac{64}{63} + \log. \frac{\overline{63}|^2}{\overline{63}|^2 - 1} = \log. \frac{32}{31}$. Here the fraction, whofe odd powers are to be ufed in the feries, is $\frac{1}{7937}$, and the very firft term of it, will give the logarithm true to twelve places of figures.

Again, if the logarithm of $\frac{16}{15}$ were to be computed from that of $\frac{32}{31}$ found above, we fhould have $2 \log. \frac{32}{31} + \log. \frac{\overline{31}|^2}{\overline{31}|^2 - 1} = \log. \frac{16}{15}$. Here the fraction to be ufed in the feries is $\frac{1}{1921}$, the firft term of which will give the logarithm true to ten places of figures.

In like manner, from the logarithm of $\frac{16}{15}$ we may find that of $\frac{8}{7}$; from logarithm of $\frac{8}{7}$ that of $\frac{4}{3}$; and from the logarithm of $\frac{4}{3}$ that of $\frac{2}{1}$, as is done above. The refpective fractions for the feries will be $\frac{1}{449}$, $\frac{1}{97}$, and $\frac{1}{17}$.

Thus far the fractions I have taken have even numbers for their numerators; let us now take one whofe numerator is an odd number $\frac{9}{8}$. Here n being $= 3\frac{1}{2}$, $\log \frac{9}{7} \left(\frac{4\frac{1}{2}}{3\frac{1}{2}}\right) = 2 \log. \frac{9}{8} + \log. \frac{64}{63}$; and the fraction whofe odd powers are to be ufed is $\frac{1}{127}$. Hence we have the log. of $\frac{8}{7}$ (for $\frac{9}{7} \div \frac{9}{8} = \frac{8}{7}$) and may proceed to find the logarithm of 2 as above. But the logarithm of $\frac{8}{7}$ may be directly derived from the equation thus: the equation in other terms is, $\log. 9 - \log. 7 = 2 \log. 9 - 2 \log. 8 + \log. \frac{64}{63}$; then, by tranfpofition, $\log. 8 - \log. 7 = \log. 9 - \log. 8 + \log. \frac{64}{63}$, or $\log. \frac{8}{7} = \log. \frac{9}{8} + \log. \frac{64}{63}$.

But when the numerator of the fraction, whose logarithm is given, is odd, theorem 2. is more commodious. For taking $\frac{9}{8}$, as before, we have $2 \log. \frac{9}{8} - \log. \frac{81}{80} = \log. \frac{5}{4}$, where the fraction to be involved is $\frac{1}{161}$. Again, $2 \log. \frac{5}{4} - \log. \frac{25}{24} = \log. \frac{3}{2}$, where the fraction is $\frac{1}{49}$. And $2 \log. \frac{3}{2} - \log. \frac{9}{8} = \log. \frac{2}{1}$, where we have only to take the difference of logarithms, as the logarithm of $\frac{9}{8}$ as well as that of $\frac{3}{2}$ is given.

All the above calculations are of hyperbolic logarithms; but the same theorems hold good for Mr. BRIGGS's, or any other. I will give an example in the computation of BRIGGS's logarithm of 7 from others already known.

Let the logarithms of 100, 99, and 50, be given; then (by theorem 1.) $2 \log. \frac{100}{99} + \frac{\overline{99}|^2}{\overline{99}|^2 - 1} = \log. \frac{50}{49}$, or $\log. \frac{100}{99} + \frac{1}{2} \log. \frac{\overline{99}|^2}{\overline{99}|^2 - 1} = \frac{1}{2} \log. \frac{50}{49}$; and then $\frac{1}{2} \log. 50 - \frac{1}{2} \log. \frac{50}{49} = \frac{1}{2} \log. 49 = \log. 7$.

Log. of $\frac{100}{99}$ - - -	0·0043648054024 5
$\frac{1}{2}$ log. of $\frac{\overline{99}|^2}{\overline{99}|^2-1}$ $\left(= \frac{0{\cdot}43429,\ \&c.}{19601} = \right)$	0·0000221567512 8
$\frac{1}{2}$ log. of $\frac{50}{49}$ - - -	0·0043869621537 3
$\frac{1}{2}$ log. of 50 - -	0·8494850021680 1
Log. of 7 - -	0·8450980400142 8

SCHOLIUM.

SCHOLIUM.

Neither the number 2, nor the fraction $\frac{64}{63}$ is chosen as the most advantageous to begin with in computing a table of logarithms; but they are taken as some of the first that occurred, to show the use of these theorems. Perhaps there are other instances in which they would be shown to much more advantage; but I hope their use will appear from the few examples given. They may indeed be transformed so as to be more commodious in particular cases, and there may be some others derived from them, one or two of which I will here put down.

It is evident from theorem 1. and 2. that 2 log. $\frac{2p+2q}{2p+q}$ + log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$ = 2 log. $\frac{2p+q}{2p}$ − log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$; consequently, 2 log. $\frac{2p+2q}{2p+q}$ + 2 log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$ = 2 log. $\frac{2p+q}{2p}$, or, log. $\frac{2p+q}{2p}$ = log. $\frac{2p+2q}{2p+q}$ + log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$, which may be called theorem 3.

Again, this equation may be thus expressed: log. $\overline{2p+q}$ − log. $2p$ = log. $\overline{2p+2q}$ − log. $\overline{2p+q}$ + log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$; and, by transposition, 2 log. $\overline{2p+q}$ = log. $\overline{2p+2q}$ + log. $2p$ + log. $\frac{\overline{2p+q}|^2}{\overline{2p+q}|^2-qq}$, which may be called theorem 4. And this is, in effect, one of the theorems given by Dr. HAL-

LEY,

LEY, in the Philoſophical Tranſactions, N° 216, and of which the doctor ſaid, it converges ſo very faſt, that, in his opinion, nothing better was to be hoped.

There are yet ſome contrivances different from thoſe mentioned in the beginning of SHERWIN's book of mathematical tables, or any other that have come to my hands, whereby the labour of computing a table of logarithms is ſhortened; but to explain them would require more time than my preſent ſituation affords me.

The obſervations and reaſonings which led me to the diſcovery of the above theorems, I imagine, need not here be mentioned. Such as they are, I beg leave to lay them before the candid and ſkilful in theſe matters, in hopes that the invention will appear to them, as it does to me, a uſeful one.

It has, indeed, been objected, by a gentleman of my acquaintance, that improvements in the conſtruction of logarithms cannot now be uſeful, becauſe logarithms are already conſtructed.

I anſwer, that argument, if it has any weight, operates equally againſt Sir ISAAC NEWTON, Dr. HALLEY, Mr. COTES, Mr. SIMPSON, and ſeveral other ingenious mathematicians; for logarithms were invented, and tables of them conſtructed, before their time; ſo that if I ſhould be thought to have miſemployed my time in

improving

improving the computation of theſe artificial numbers, I have ſome conſolation in thinking that I have therein followed the example of the very reſpectable company juſt mentioned.

I truſt, however, that, with mathematicians, every improvement in calculation will be acceptable.

XVII. *Connoiſſances eſſentielles pour juger de quelque Eſpèce nouvelle de Moulin à Cannes qu'on puiſſe propoſer. Par Monſieur* Cazaud, *Membre de la Société Royale.*

Read March 16, 1780.

SI l'on exigeoit d'un homme qui a une maſſe conſiderable à déplacer, qu'il en connut exactement le poids, avant d'y appliquer le levier, on exigeroit ſouvent une choſe impoſſible; mais la maſſe une fois ſoulevée avec le dixieme levier, ſi les neuf premiers n'ont pas réuſſi, on ſçait la reſiſtance qu'il faudra deſormais lui oppoſer.

Les premiers moulins à ſucre ont été faits ſans principes; cela devoit être: il eſt vray qu'on y a fait peu ou point de changements eſſentiels; on en propoſe tous les jours qu'on regarde comme tels, et qui dans la pratique ſe reduiſent à peu de choſe. Je crois pouvoir dire ſans indiſcretion que le peu de mecaniciens que j'ay vu diriger leurs vues vers cet objet, n'ont pu me répondre lorſque je leur ai demandé à combien ils evaluoient la reſiſtance des cannes: j'avouerai avec la même franchiſe que j'avois

j'avois fait moi-même pluſieurs expériences très couteuſes et fort inutiles, ſans avoir pris la peine de repondre à ma queſtion, dont je ne ſentois que machinalement l'importance: ſur quel principe les autres agiſſoient-ils? ſur quel principe avois-je moi-même agi?

On conſtruiſit à Londres il y a onze ans une machine à feu deſtinée auſſi à preſſer les cannes: l'auteur m'en parla; je lui fis ma queſtion ordinaire, il me repondit qu'il evaluoit la reſiſtance à ſix milliers: ſa réponſe qui préſentoit une idée préciſe m'autoriſa à le prier de me communiquer les fondemens de cette evaluation; il me repondit qu'il etoit impoſſible que la reſiſtance fut plus conſidérable qu'il ne l'avoit ſuppoſée; je le priai de me permettre de refléchir ſur une idée qui me vint dans ce moment la, et qui me paroiſſoit meriter d'être approfondie. La voici; j'etablirai des faits, il ſera facile d'apprecier mes conſequences.

On connoît le mecaniſme de nos moulins à beſtiaux, ils ont ordinairement de 45 à 55 pieds de diametre, ſuppoſons 50. On applique deux mulets à environ un pied de chacune des extremités d'un bras qui traverſe le grand role auquel eſt attaché le cylindre du milieu; ce cylindre a 17 ou 18 pouces de diametre; la reſiſtance des cannes ſe trouve donc à neuf pouces du centre de l'action; il faut pour la vaincre un effort continu de quatre mulets

appliqués

appliqués à un levier d'environ 24 pieds; cet effort equivaut à 600 livres, à raiſon de 150 par mulet. Dans 24 pieds il y a 32 fois neuf pouces, ou 32 fois 600 livres; donc la reſiſtance des cannes eſt d'environ 19 milliers dans un moulin à beſtiaux; car il faut eſſentiellement remarquer qu'après une demie heure de travail les quatre mulets ſont en eau, et qu'on ne les change que de deux heures en deux heures.

Après cet expoſé l'auteur de la machine à feu me dit qu'il pouvoit facilement tripler, quadrupler même, ſa puiſſance ſi cela etoit neceſſaire; il ne me demanda point d'autres details, qui m'auroient ſans doute ſuggéré dès alors quelques idées qui me ſont venues depuis; à l'inſpection d'un autre moulin propoſé je perdis de vue la machine à feu; deux ans après on me dit qu'on l'avoit envoyée à la Jamaique, j'en ignore le ſuccés, que la renommée auroit probablement fait connoitre ſi elle eut repondu aux idées de l'auteur.

Voici quelques autres obſervations qu'il faudroit joindre à la connoiſſance preliminaire et, je crois, eſſentielle que je viens de donner.

S'il ne s'agiſſoit que de vaincre (n'importe en combien de tems) la reſiſtance de 19 milliers dont j'ay parlé, on conçoit qu'en appliquant l'effort continu de deux hommes qu'on évalue à 50 livres, au bout d'un levier de 388

pieds, on auroit un produit egal à celui des quatre mulets du moulin ordinaire, mais alors on n'obtiendroit qu'en douze heures, ce qu'on obtient dans une feule avec les quatre mulets.

Il faut fçavoir auffi que ce moulin à beftiaux, au quel on eft obligé de confacrer au moins 36 mulets, ne donne cependant, une heure dans l'autre, qu'environ 80 à 100 gallons de liqueur, et qu'un bon moulin à l'eau, tel qu'il le faudroit pour faire ce qu'on appelle rondement 250 à 300 bariques de fucre dans la bonne faifon, doit donner 160 à 200 gallons de liqueur par heure, l'une dans l'autre.

Il faut fçavoir auffi que pour donner ces 160 gallons de liqueur dans le tems fec de Mars ou d'Avril, les cylindres qui preffent les cannes doivent tourner deux fois et demi dans une minute, comme pour en donner deux cent dans le mois de Janvier.

Il faut remarquer auffi que la difference du produit des deux moulins dont je viens de parler, fuppofant neceffairement une différence egale foit dans les refiftances vaincues, foit dans les tems employés à les vaincre; la refiftance qu'il y auroit à vaincre dans un nouveau moulin qu'on auroit deffein de rendre equivalent à un excellent moulin à l'eau, devroit donc être fuppofée d'environ 38 milliers, ou bien la puiffance deftinée à la vaincre, devroit

par-

parcourir dans une heure, l'eſpace que les mulets du moulin ordinaire à beſtiaux, parcourent en deux.

Si le peu de principes que je viens d'établir, euſſent été connus, il eſt probable que les auteurs de beaucoup de moulins propoſés n'euſſent pas perdu leur tems à les imaginer, ou que differentes Academies en euſſent moins perdu à en faire l'examen.

XIX. *Account of an Offification of the Thoracic Duct.* *By* Richard Browne Chefton, *Surgeon to the Infirmary at* Gloucefter; *communicated by Mr.* Henry Watfon, *Surgeon to the* Weftminfter Hofpital.

Read April 20, 1780.

JAMES JONES, twenty-two years of age, was admitted into the Gloucefter Infirmary, June the 5th, 1779, for very troublefome pains in his back and hip, which, from every circumftance of his defcription, were fuppofed rheumatic.

Upon a particular examination a few days afterwards, the right hip was obferved to be fuller than it fhould be; but the thigh feemed very little altered from its natural ftate. A blifter was applied over the joint, and the ufual anti-rheumatic remedies were prefcribed by the phyfician under whofe care he was admitted. In about a fortnight he complained of a violent pain in his knee, for which another blifter was ordered to the head of the fibula. During this time he could move about the ward

by the aſſiſtance of a ſtick, but ſoon after not without crutches.

His thigh now increaſed in bulk, and became œdematous; for which reaſon another bliſter was applied about the middle of it, and his knee getting into a contracted ſtate, a volatile liniment was rubbed on that part. As he now could not move about, even with the aſſiſtance of crutches, he took to his bed altogether.

From this time the enlargement of his thigh advanced very faſt, and his knee became contracted in proportion; ſo that the thigh had got into the ſame kind of relaxed poſition the limb is frequently placed in when the bone is fractured.

Soon afterwards he began to find ſome difficulty in the diſcharge of his urine, which by degrees increaſed ſo much, that the medicines preſcribed for his relief in this particular, not having the deſired effect, and his belly appearing to be diſtended from this cauſe, a catheter was attempted to be introduced, but it could not be made to paſs the neck of the bladder: a bougie, however, entered the bladder with eaſe, and ſome water came off upon withdrawing it.

From this period, by means of very great exertions of the abdominal muſcles, and by occaſional preſſure externally, he uſed to diſcharge his urine; but it came away

in small quantities only at a time, and seemed to empty the bladder but very little; for a tumor, which for many days had been perceptible on the left side, and which evidently contained a fluid, afforded the usual feel of a distended bladder.

He now suffered very violent pains all over the abdomen, but particularly about the region of the pubis, so that he could not bear the pressure usually made for some time past to force off his urine; and, as the catheter could not enter the bladder, and the introduction of the bougie was of very little service, his urine now began to dribble away involuntarily.

His fever, at intervals, was very considerable, his strength failed him very fast, and he received no benefit from any medicines but opiates. In this most deplorable condition he languished till the beginning of October, when the violence of his pains began to remit, and he gradually drooped into a state of insensibility till the tenth of October, when he died.

DISSECTION.

The integuments of the abdomen felt harsh and dry, like a piece of parchment. The veins were much enlarged, and their branches could be traced all over its

ſurface in a very diſtinct manner. On the left ſide there ſtill remained an evident fullneſs, which puſhed the integuments forwards to the ſize of two fiſts, and which contained a fluid in conſiderable quantity.

The thigh ſtill continued in the ſame poſition and œdematous ſtate before deſcribed, ſo that from thoſe circumſtances, and the diſtenſion of the abdomen, nothing particular could be obſerved externally, at the lower part of the abdomen, on the right ſide more than on the left.

Upon examining the cavity the inteſtines preſented themſelves nearly in their natural ſituation: their appearance was ſound, but in general they were much inflated. The tumor on the left ſide proved to be the bladder diſtended with urine, ſlightly adhering to the peritoneum; and this, together with the colon paſſing on to its termination in the rectum, filled up the iliac region on that ſide, while the right ſide, and indeed more than half the pelvis, was fully occupied by a confuſed irregular maſs, ſeemingly formed of ſcirrhous cartilage, bone, and ſtone.

As a large cartilaginous ſubſtance [(a)] ariſing from this maſs ſeemed to cover the bodies of the vertebræ, I removed the inteſtines, and purſuing this ſingular appear-

(a) This ſubſtance, when firſt taken out of the body, appeared cartilaginous; but when dried was perfect bone.

ance,

ance, traced it upwards in the courfe of the fpine, and of the large blood veffels, to its termination fomewhat above the kidneys.

Upon laying bare the fternum and ribs, to infpect the cavity of the thorax, the cartilages prefented themfelves in the whiteft ftate I ever faw, approaching nearly to the colour of writing paper, ftill retaining their natural firmnefs and texture.

The lungs were in a full ftate of diftenfion, and ftudded in many parts with the fame kind of cartilaginous fubftance which appeared fo plentifully in the lower cavity.

Sufpecting that the arterial fyftem might, in fome meafure, be affected by this prevailing difeafe, I feparated the heart from the lungs, for the purpofe of examining its larger fyftem of veffels, and, dividing the aorta juft below its curvature, found, upon examination, the heart very flaccid, empty, and of a much fmaller fize than ufual in adults of the age of this patient, but found in every refpect.

The femi-lunar and mitral valves were not at all difeafed, nor was there the fmalleft deviation in any part of the aorta from the moft healthy ftate, though it was intirely furrounded by this fingular fubftance from the

paffing

paſſing off of the cœliac artery to where it bifurcates into the two iliacs.

On raiſing the aorta from the ſpine, after I had divided it at its curvature, I found a ſingular firmneſs on its right ſide, like a piece of hard pack-thread, and exactly in the ſituation of the thoracic duct. I continued my diſſection therefore with great caution, and at ſuch an extent as to take in the vena azygos, and afterwards found upon clearing it at my leiſure, that it really was the thoracic duct, intirely plugged up with oſſific matter, from immediately above the receptaculum chyli.

Unfortunately, being much ſtreightened for time when I opened the body, I was obliged to take out the parts for a more careful examination at home, which deprived me of the opportunity of aſcertaining the above circumſtances at the time I ſeparated the parts, and conſequently of inquiring how much further up this ſingular oſſification extended, and in what ſtate the duct might be at its entrance into the ſub-clavian vein. The vena azygos, as well as the aorta, was perfectly ſound.

The vena cava was not ſo free from diſeaſe; for though it bore externally a natural appearance, when I laid it open from finding a ſingular feel within, I found that its cavity was above half filled with a firm inelaſtic ſubſtance: this

this fubftance originated from its internal furface near the entrance of the emulgent vein, attached to it here and there by fmall points, till about the projection of the facrum, where the cavity of the vein was almoft filled up with a continuation of the fame fubftance.

The fpleen, pancreas, and liver, were perfectly found: the gall bladder very fmall, and quite empty; its ducts in a natural ftate.

The kidnies were much increafed in their fubftance, externally more livid than ufual, and feemingly in a ftate of inflammation. The ureter on the right fide was much enlarged, and contained a confiderable quantity of urine, which feemed retained there by the diftenfion of the bladder. The left ureter was of its natural fize and appearance. The coats of the bladder were confiderably thickened, but preferving externally as well as internally its moft healthy appearance.

It may be neceffary here to remark, that as the bladder could not expand itfelf laterally, it was extended upwards in an oblong form, not unlike that of a calf's, but did not appear capable of containing more than a quart.

The tumor which occupied the right iliac region, extended itfelf irregularly in all directions, and appeared outwardly to have deftroyed or brought on an abforption

of

of a principal part of the os innominatum, fo far as this could be afcertained by thrufting a probe into it in different directions.

The manner in which a portion of this tumor preffed on the neck of the bladder readily accounts for the difficulty of paffing the catheter, though a bougie eafily flipped in, and gives us a reafon why the patient was unable to empty his bladder for fo long a time before his death.

The degree of injury the os innominatum has fuftained cannot yet be certified, the tumor not being fufficiently reduced by maceration. Where the bone is cleared from the furrounding foft parts, it appears to have fuffered a great lofs of fubftance, and, as the tumor diffolves, a large quantity of bony matter now deprived of its connecting medium is continually fubfiding to the bottom of the veffel in which the tumor is macerating.

The preparation of the thoracic duct was at firft put up in fpirits to preferve its original appearance, and in this ftate I brought it with me to London. Upon fhewing it here to feveral anatomical gentlemen, they were in doubt whether there might not be yet remaining fome fmall cavity in the duct, and were therefore defirous I fhould take the preparation out of the fpirits to be examined more accurately. This I readily complied with, and

and it was accordingly examined very circumſtantially by Dr. HUNTER, Mr. WATSON, Mr. J. HUNTER, and Mr. CRUIKSHANKS, who honoured me with a viſit for that purpoſe.

The appearances which theſe gentlemen particularly noticed were, that the duct was completely filled up, excepting at the lower bulbous part, commonly called the receptaculum chyli, where, indeed, there was room enough for air to paſs between the coat of the duct and the adventitious ſubſtance within it; ſo that the receptacle, which before appeared flat, upon throwing in air became rounded and fully diſtended: but this air was totally confined to the receptacle, and could not be forced up the duct in the ſmalleſt degree. The receptacle was then ſlit open, and an attempt made to paſs a briſtle up the duct, but this was found impoſſible.

Mr. CRUIKSHANKS afterwards endeavoured to force mercury up the duct, but not the ſmalleſt particle would paſs.

From theſe different examinations we were all thoroughly convinced, that the receptaculum chyli was not ſo completely filled up but that it might receive a ſmall quantity of fluid, yet the duct itſelf was totally impervious, without a poſſibility of admitting any.

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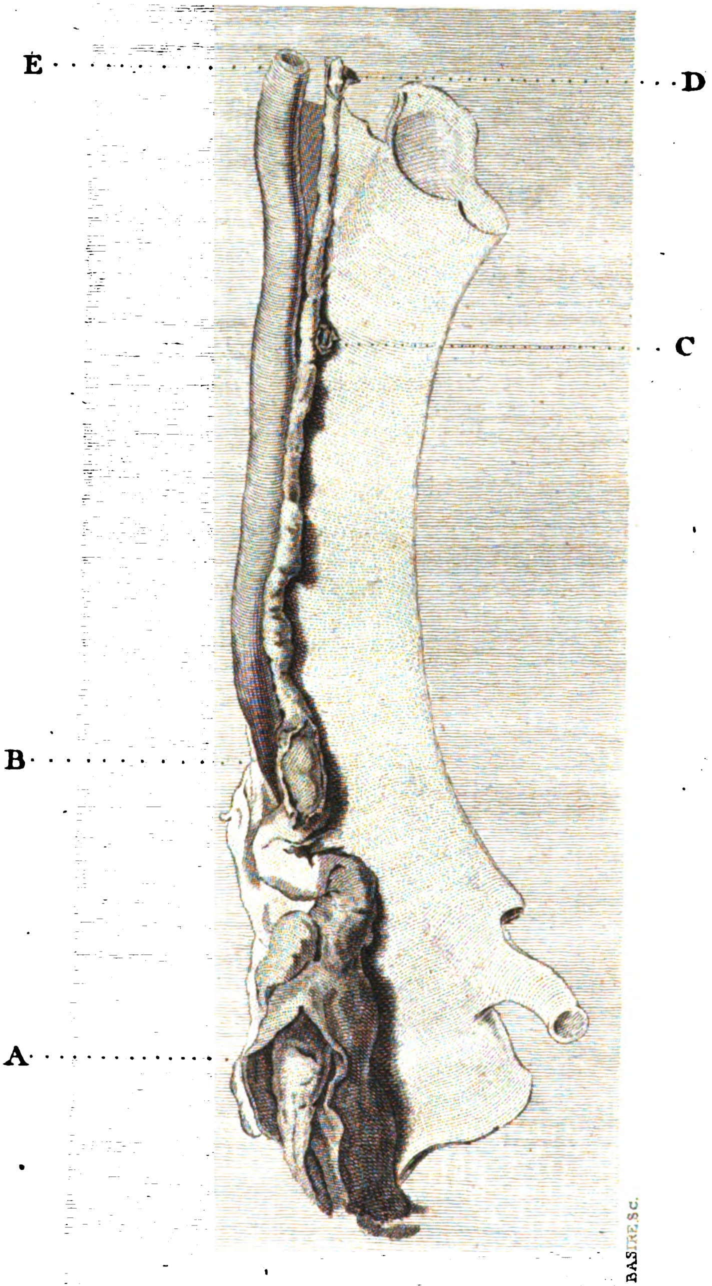
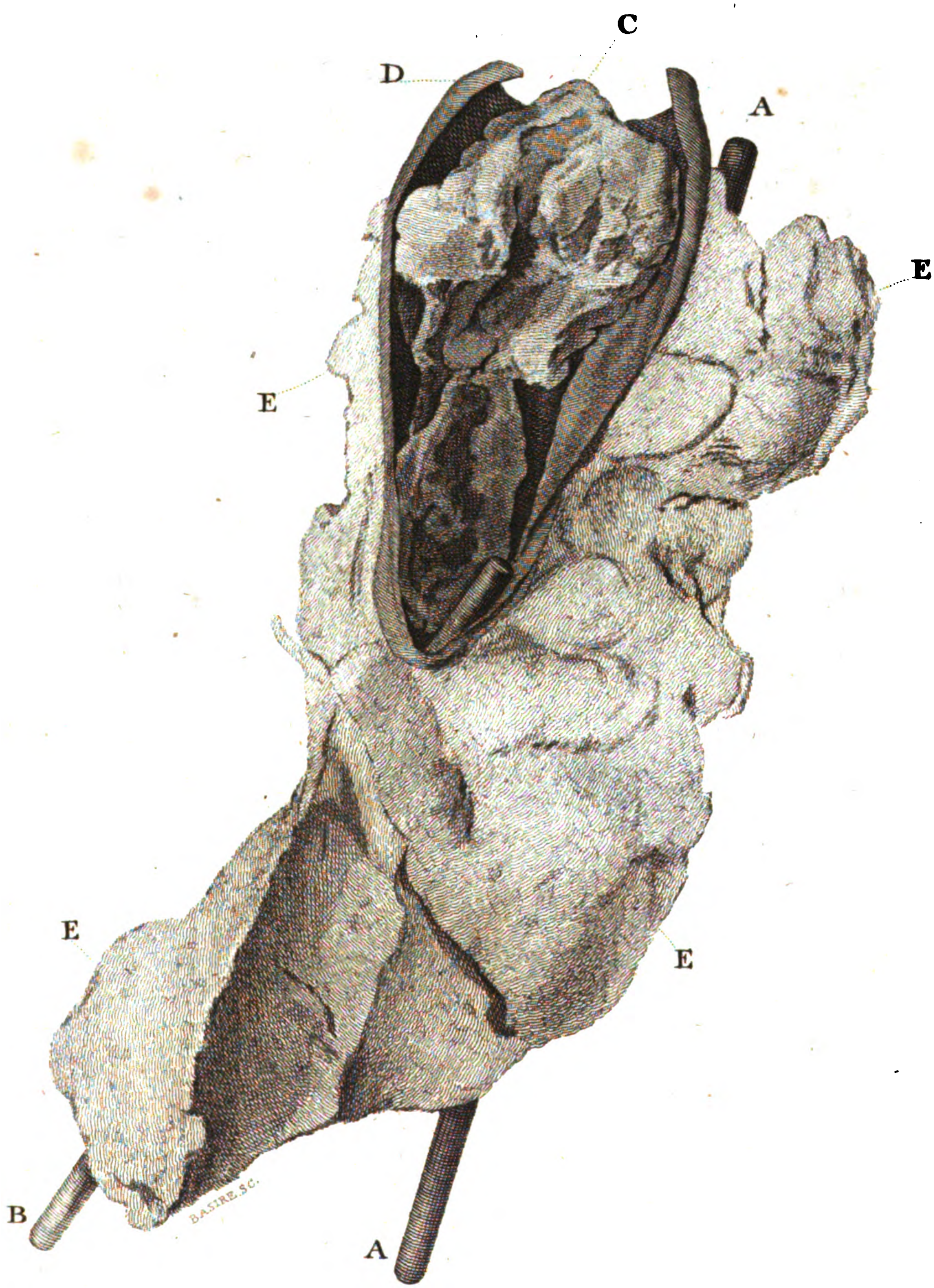
The coats of the duct did not appear to have undergone any morbid change: for in some places where the substance it contained was not so strongly attached but that the coat would admit of being raised from it, they were found in a perfect natural condition. At other places, where the attachment was inseparable, there was a greater appearance of ossification externally; but this we were convinced arose merely from the thinness of the coats in that part, where the receptaculum chyli was laid open, the substance within it appeared of a membraneous nature very similar to that found in the vena cava of this same subject, but more laminated. We presumed, that the same kind of membrane had been continued through the whole of the duct, but was now become pretty completely ossified in all that portion of the duct which we were in possession of.

A small body, resembling a lymphatic gland on the side of the upper part of the duct, was opened by Mr. J. HUNTER, who found the same ossific disposition in this little body, as we before noticed in the duct itself.

We next examined the substance that partly filled up the vena cava. It was in length about four inches; at the upper end, broad and conical; at the lower, much narrower and rather rounded. Its surface appeared irregular and granulated with small bony particles. It



A. The Receptaculum Chyli laid open to shew the ligamentous Substance within it.
B. An Opening into the Thoracic Duct to shew the manner in which it was block'd up.
C. The little lymphatic Gland containing a similar Substance to that in the Receptac. Chyli.
D. The Coats of the Duct seperated from the bony Matter with which it was completely fill'd.
E. Vena Azygos.



A. *A Bougie in the descending Aorta; the lower point having pass'd into the left Iliac.*
B. *A red Bougie in the lower part of the Vena Cava.*
C. *The Ligamentous Substance filling up the Vena Cava.* D.
E. *The boney Matter surrounding the Aorta & Vena Cava.*

appeared flattened, perhaps from the preſſure of the blood conſtantly moving over it; for having made a ſmall opening with a lancet into the narrow part of this ſubſtance, we could paſs a probe very readily both upwards and downwards, ſo as to convince us, it was really hollow all the way. Upon introducing a blow-pipe, the upper part was expanded into a large *cul de ſac*, and the lower diſtended pretty much like a large veſſel. The appearance of this man during his illneſs, as well as at the time of his death, was exactly ſimilar to what I had frequently obſerved in patients who had lingered under, and been deſtroyed by, ſlow inflammations of the viſcera. His complaints in the abdomen only indicated a diſeaſed bladder, and, for that reaſon, I opened him: for he was not even ſo much emaciated as we often find patients under that diſeaſe.

XX. *An Account of the Effect of Electricity in shortening Wires. By Mr.* Edward Nairne, *F. R. S.*

Read May 11, 1780.

EVERY new fact in a ſcience being an acquiſition to it, I beg leave to lay before this Society the following diſcovery, which I made ſome time ago, relative to electricity ſhortening wire by its paſſage through it. The following experiment I hope will clearly illuſtrate it.

EXPERIMENT.

I took a piece of hard-drawn iron wire, ten inches long, and one hundredth of an inch in diameter. This wire was held in a perpendicular poſition between two braſs pinchers, the upper one of which was connected with a glaſs pillar, in order that the whole charge of an electrical battery might paſs only through the wire faſtened between them. Theſe upper pinchers were moveable, for the ſake of ſlackening the wire occaſionally; and, in the experiment, they were fixed with a ſcrew,

a ſcrew, ſo that the wire hung ſomewhat looſe between them and the lower ones. I then charged a battery, containing twenty-ſix feet of coated ſurface, till the index of the electrometer was raiſed to fifty degrees: it was then diſcharged through the wire, and, immediately after, the wire was ſeen to ſhorten by its drawing nearer to a ſtraight line between the fixed pinchers. If the wire was put ſo looſe that the moving of the upper pinchers three quarters of a tenth of an inch, or ſeventy-five thouſandths, would draw it juſt ſtraight, one diſcharge of the battery through it would then draw it to a ſtraight line. I diſcharged the ſame battery nine times through a piece of the ſame wire, which was alſo of the ſame length, which was ſlackened each time before the diſcharge went through it. There were preſent Sir JOHN PRINGLE, the hon. Mr. CAVENDISH, Mr. SMEATON, the Rev. Mr. MICHEL, Mr. RUSSEL, and Mr. WHITEHURST. After the ſixth and ninth time Mr. SMEATON meaſured the wire, and found it to have ſhortened in the proportion of three quarters of a tenth of an inch each time.

I afterwards diſcharged the battery ſix times more through the ſame piece of wire, which made fifteen times in the whole, and found it had continued ſhortening nearly in the ſame proportion, the wire having been ſhortened by the fifteen ſtrokes full one inch and one tenth,

tenth, *viz.* its whole length being now barely eight inches and nine tenths inftead of ten inches that it was at firft. I then weighed it in a pair of fcales that would turn with lefs than the eighth part of a grain; but could not perceive, that there was any fenfible difference in the weight. I tried it with a pair of callipers, and it feemed to be rather thicker.

I intended to have paffed feveral more difcharges of the battery through the wire, but the fixteenth melted it.

I have likewife fhewn this extraordinary effect of electricity to Dr. PRIESTLEY, Mr. MAGELLAN, and feveral other gentlemen, in whofe prefence I paffed fix difcharges of the fame battery through a piece of the fame wire, exactly ten inches in length; and on meafuring it, it was found to be fhortened four tenths and a half of an inch, which is juft three quarters of a tenth for each difcharge. Dr. PRIESTLEY then took a piece of the fame wire, of exactly the fame length, and heated it red-hot in the common culinary fire. This wire being afterwards meafured was found to continue of its original length of ten inches, not being in the leaft fhortened.

I have generally found, that if the iron wire was firft annealed in the culinary fire, the fame ftrength of charge melted it.

I have

I have alſo tried a piece as copper wire, gilt with ſilver, and of the ſame dimenſions as the iron wire, and find, that the ſame charge, diſcharged through it, ſhortens it one twentieth of an inch, *viz.* two-thirds of what the iron wire was ſhortened. The ſame ſtrength of electrical charge, as near as could be meaſured by an electrometer, which paſſing through the iron wire, made it viſibly red-hot in a bright day, the ſun ſhining at the ſame time, did not heat a piece of copper wire of the ſame dimenſions ſo as to appear of a red heat, though the room was made dark. If the battery was but a little more charged, the iron wire then would be melted; but it had no ſuch effect on the copper wire.

This ſeems to point out, that iron wire reſiſts the paſſage of the electric fluid much more than copper; and alſo, that the culinary fire and electrical fire have different effects on iron and copper: for malleable iron, I am informed, is one of the moſt difficult metals to melt by the culinary fire, and requires a much greater heat to melt than copper; whereas, on the contrary, the iron is melted with a much leſs charge of electric fire.

XXI. *Aſtronomical Obſervations on the periodical Star in Collo Ceti*[a]. *By Mr.* William Herſchel, *of* Bath; *communicated by Dr.* Watſon, *Jun. of* Bath, *F. R. S.*

Read May 11, 1780.

THIS remarkable ſtar, we are told[b], "was firſt ob-"ſerved by DAVID FABRICIUS, the 13th of Au-"guſt, 1596, who called it the ſtella mira, or wonder-"ful ſtar; which has been ſince found to appear and "diſappear, periodically, ſeven times in ſix years, conti-"nuing in the greateſt luſtre for fifteen days together, "and is never quite extinguiſhed."

My own obſervations on this wonderful ſtar are but few, yet ſufficiently verify the ſurprizing appearances that have been aſcribed to it. I ſhall tranſcribe them from my aſtronomical journal in the order they were made.

October 20, 1777, I looked out for the periodical ſtar in collo ceti, but it was not viſible. If its period is 312

(a) BAYER's character for this ſtar is *o*.
(b) See FERGUSON's Aſtronomy. § 366.

days I may expect to see it about Christmas, not being visible at present.

Dec. 18, 1777, I saw the periodical star in collo ceti. It appeared in the very place where, about a fortnight ago, I imagined (but was not sure) there was a faint appearance of it. It was in magnitude about equal to ζ, but not so large as δ[(c)].

Jan. 26, 1778, The periodical star was larger than δ, but less than ν. Being taken up with other observations, I paid no more attention to it during the rest of this period.

Sept. 18, 1779, The periodical star was visible to the naked eye, when I first looked for it.

Oct. 6, 1779, The periodical star was exceedingly bright this evening. It exceeded α and β ceti; which latter, I must here observe, is considerably larger than the former, and affords a proof of the change in the magnitude of the fixed stars; as we can hardly suppose BAYER should have made a mistake in the magnitude of the two first stars of this constellation.

The apparent magnitude of *o* ceti was not round but elliptical, when observed through the telescope, and not very well defined; but as it was too near the horizon,

(c) ζ is marked by BAYER as a star of the fourth magnitude, and δ of the third.

this ſhape might ariſe from that cauſe, for other low ſtars were alſo irregular in their forms, yet bellatrix was exceedingly fine and quite round.

Oct. 7, 1779, The periodical ſtar was perfectly round in the teleſcopes, and its apparent diameter well defined, full, and very large, for a ſtar of that magnitude.

When I ſpeak of the apparent diameter, I would be underſtood to diſtinguiſh it not only from the real diameter, but alſo (if I may be allowed the expreſſion) from the real apparent diameter. To explain this a little more at large: the body of the ſun, for inſtance, is of a certain dimenſion which we call his real diameter, and this remains always the ſame. His apparent diameter (which I here call real apparent) is changeable, according as we approach to, or recede from, him, and is between 31′ 33″. and 32′ 39″; but were he removed to the diſtance of one of the neareſt fixed ſtars, neither his real, nor real apparent diameter, could then be known to us by any method we have hitherto been acquainted with: for at the diſtance of at leaſt 20 billions of miles, his real apparent diameter could not much exceed thirty fourths of a degree; and a teleſcope muſt magnify above fourteen thouſand times to make him appear of only two minutes in diameter, which ſtill is hardly ſufficiently large to diſtinguiſh a ſquare from a circle: and yet I doubt not, but

4 that

that we ſhould obſerve ſome apparent diameter or other of the ſun thus removed from us; and this is what I here have called the apparent diameter. This muſt be owing to ſome optical deception. DE LA LANDE explains it thus: " Si l'on voit dans les lunettes une lumière éparſe " qui environne les étoiles, qui les amplifie et les fait " paroitre come ſi elles avoient 5 a 6″ de diametre, on " doit attribuer cette apparence à la vivacité de leur lu- " mière, à l'air environnant et illuminé, à l'aberration des " verres, à l'impreſſion trop vive qui ſe fait ſur la rétine."

Oct. 19, 1779, The periodical ſtar preceded a very obſcure teleſcopic ſtar at the diſtance of 1′ 45″.16. This meaſure was extremely difficult to take, the ſmall ſtar being ſo obſcure as hardly to bear the field of view to be ſufficiently enlightened: both alſo having nearly the ſame declination, they therefore paſs the parallel hairs of the micrometer almoſt at rectangles. However, I ſhould ſuppoſe, the meaſure muſt be true to 3 or 4″.

I meaſured the diſtance once more, with a better light and more ſatisfaction to myſelf, and found it 1′ 50″.47.

12 o' clock, The periodical ſtar is now about the meridian, and brighter than α arietis.

Oct. 30, 1779, 9 o'clock, The periodical ſtar is ſtill increaſed, and viſibly larger than α arietis, though not in the meridian at preſent.

 —— o'clock,

— o'clock, *o* ceti being now in the meridian, is almoſt of a middle ſize between aldebaran and α arietis. Its apparent diameter by the teleſcope is alſo increaſed.

Nov. 2, 1779, The luſtre of the periodical ſtar is ſtill increaſed. The body is very full and round in the teleſcope. I magnified it 449 times very diſtinctly, the evening being ſo fine; but my uſual power is only 222.

Nov. 20, 1779, The periodical ſtar ſeemed to be as bright as before, but no brighter.

Nov. 30, 1779, *o* ceti is conſiderably decreaſed. Its magnitude is leſs than β but greater than α. I have before remarked, that α is leſs than β.

Dec. 4, 1779, The luſtre of *o* ceti is only equal to α. I meaſured the diſtance of this ſtar from the obſcure one which follows it:

Firſt meaſure,	1′ 53″.437
Second meaſure,	1′ 50″.625

The weather was ſo cold, that I could hardly finiſh this laſt meaſure; but I believe it to be too ſmall. The diſagreement is owing to the difficulty, and not to want of accuracy in the micrometer.

Jan. 4, 1780, The periodical ſtar is very much diminiſhed. I took the diſtance, but the evening was very indifferent, and therefore the ſmall ſtar ſo faint as to allow but little light. It meaſured 1′ 45″.937. I thought the

the meaſure too little when I took it, but could not ſucceed better.

Feb. 7, 1780. The periodical ſtar was inviſible to the naked eye. I was but little prepared to look a long time for it with the teleſcope; but ſuppoſe I ſhall be able to find it another time.

MAUPERTUIS accounts for the periodical appearances of changeable ſtars, by ſuppoſing that they may be of a flat form, like Saturn's ring, which becomes inviſible when the edge is preſented to us.

As the periodical ſtar in collo ceti appeared always full and round when I viewed it with a teleſcope, this might at firſt appear to contradict the ſuppoſition of MAUPERTUIS; but, upon proper conſideration, will be found not to be at all againſt it: for, ſuppoſe the real apparent diameter of this ſtar to be one-third of a degree; then, ſince it appeared to me (I did not meaſure it) at leaſt of one ſecond, when at the full, it will follow, that there was an aberration whatever might be the cauſe of it, which amounted to 59‴, by which its real apparent diameter was increaſed from 1‴ to 1″. Now, if this ſtar, in one certain poſition, ſhould preſent its circular diſk of 1‴ in diameter, and in another ſituation only its flat edge which would appear as a line of 1‴ in length, both appearances, with the aberration included will ſtill remain

remain of a circular form: for, adding the aberration $59'''$ to the length $1'''$, the whole becomes $1''$; and adding $59'''$ to ſcarce any breadth at all, the whole breadth will alſo become nearly $1''$.

KEILL ſays, " It is probable, that the greateſt part of " this ſtar is covered with ſpots and dark bodies, ſome " part thereof remaining lucid; and while it turns about " its axis, does ſometimes ſhew its bright part, ſome- " times it turns its dark ſide to us; but the very ſpots " themſelves of this ſtar are liable to changes, for it does " not every year appear with the ſame luſtre. Some- " times it reſembles a ſtar of the ſecond magnitude; in " other years in can ſcarcely be reckoned among ſtars of " the third order; nor are the times of its viſiting us " always of the ſame duration."

XXII. *An Account of a new and cheap Method of preparing Pot-ash, with Observations.* *By* Thomas Percival, *M. D. F. R. and A. S. Member of the Royal Society of Physicians at* Paris, *&c.*

Read April 6, 1780.

THE agriculture society at Manchester has long recommended the making of reservoirs for the water which flows from dung-hills in farm yards. This water is strongly impregnated with the salts and putrid matter of the dung-hill, and by stagnation it acquires a much higher degree of putrescence, and probably becomes proportionably more replete with salts. When thus collected and improved, it is pumped into an hogshead, which being drawn upon a sledge, or small cart, is conveyed into the meadows, for the purpose of sprinkling them with this rich manure. This important improvement in rural oeconomy, I apprehend, has not been extended much beyond the district of our society; and it seems to be unknown to one of the latest and most intelligent writers on husbandry: for Lord KAIMS, in a recent

cent work on this fubject, of which he has favoured me with a copy, has not even mentioned it.

But thefe refervoirs may be applied to a purpofe ftill more fubfervient to public utility than that above defcribed. JOSIAH BIRCH, Efq. a gentleman who carries on an extenfive manufactory, and bleaches his own yarn, about fix months ago, was induced, by a happy turn of thought, to try whether the dung-hill water might not be converted into pot-afhes. He accordingly evaporated a large quantity of it, and burnt the *refiduum* in an oven; the product of which fo perfectly anfwered his expectations, that he has ever fince continued to prepare thefe afhes, and to employ them in the operations of bucking. A ftranger to that narrownefs of fpirit which feeks the concealment of a lucrative difcovery, he is defirous that it fhould be communicated to the Royal Society, and has furnifhed me with the following account, together with the plan annexed.

" The quantity of muck water ufed was twenty-four " wine pipes full, which employed a man and two " horfes two days, to cart it from the pump to the pan " wherein it was boiled; but this expence I fhall now " fave, as I fhall lay a fough of brick, which will convey " it from the pump to the boiler. The coals ufed to " boil and burn it were one hundred and twenty bafkets,

" and I ſuppoſe each baſket weighs ſix ſcore pounds or " upwards. One man was occupied three weeks in boil- " ing and burning. The quantity of aſhes made was " 9 cwt. 1 qr. 12 lbs. well worth, at the preſent price of " aſhes here, two guineas *per* hundred.

				L.	s.	d.
" 9 cwt. 1 q. 12 lb. at 42 s. *per* cwt.				19	13	0
" A man and two horſes, two " days, at 6 *s*. *per* day, -	0	12	0			
" 120 baſkets of coals, at 5 d. " *per* baſket, - -	2	10	0			
" A man's wages for three weeks,	1	7	0			
				4	9	0
				15	4	0

" The profit, therefore, amounts to 15 l. 4 s. deduct- " ing only a trifle for the wear of the pan and oven."

The profits ariſing from this preparation of pot-aſh are ſufficiently evinced by the foregoing eſtimate; and they may, perhaps, admit of increaſe by future improvements. In the ſpring and ſummer ſeaſons, I ſhould apprehend, the evaporation might be carried on without the aid of fire, by conveying the dung-hill water from the reſervoir through proper ſluices into ſhallow troughs or ponds, of ſuch extent as to afford a ſufficient

ſurface for the action of the ſun and wind[a]. Theſe might be covered in rainy weather with awnings of canvas, painted on the outſide black, and white on the inſide; the former with a view to abſorb, the latter to reflect, the rays of light.

(a) The following abridged view of a meteorological regiſter, which I kept with great exactneſs during the years 1774 and 1775, may throw ſome light on the practicability of this plan in the climate of Lancaſhire, which, I believe, is nearly the ſame as that of moſt of the other Weſtern counties of England.

1774	Thermometer		Days	
	2 o'clock.	P.M.	Rainy	Fair.
Jan. Feb. March,	56°	28°	25	65
April, May, June,	72	45	55	36
July, Auguſt, Sept.	75	53	66	26
Oct. Nov. Dec.	60	30	43	49
1775				
Jan. Feb. March,	54	30	61	29
April, May, June,	78	51	42	49
July, Auguſt, Sept.	74	48	62	30
Oct. Nov. Dec.	64	32	50*	28*

* Fourteen days omitted, no account being taken.

The thermometer, uſed in making the preceding obſervations, was graduated according to the ſcale of FAHRENHEIT. It was placed in the open air, and in a Northern expoſure. The column of rainy days expreſſes the leaſt as well as the greateſt quantity of rain. The column of fair days includes only thoſe days in which not a ſingle ſhower was noticed. The day comprehends twenty-four hours.

The mean quantity of rain which annually falls here is about 33 inches.

This

This pot-ash is of a greyish white appearance, deliquesces a little in moist air; but if kept in a dry room, near the fire, acquires a powdery surface. It is hard, and of a spongy texture when broken, with many small crystals in its substance. The colour of its internal parts is dusky and variegated. To the taste it is acrid, saline, and sulphureous. It emits no smell of volatile alkali, either in a solid form, dissolved, or when added to lime-water; neither does it communicate the sapphire colour to a solution of blue vitriol. Silver is quickly tinged black by it, a proof that it contains much phlogiston. Ten grains of this pot-ash required eleven drops of the weak spirit of vitriol to saturate them. The like quantity of salt of tartar required, of the same acid, twenty-four drops: a strong effervescence occurred in both mixtures; from the former a sulphureous vapour was exhaled. A tea-spoonful of the syrup of violets, diluted with an ounce of water, was changed into a bright green colour by five grains of the salt of tartar; but ten grains of this pot-ash were necessary to produce the same hue, in a similar mixture. Half an ounce of the pot-ash dissolved intirely in half a pint of hot water; but when the liquor was cold, a large purple sediment subsided to the bottom: and it was found, that this sediment amounted to about two-thirds of the whole quantity of ashes used.

 I have

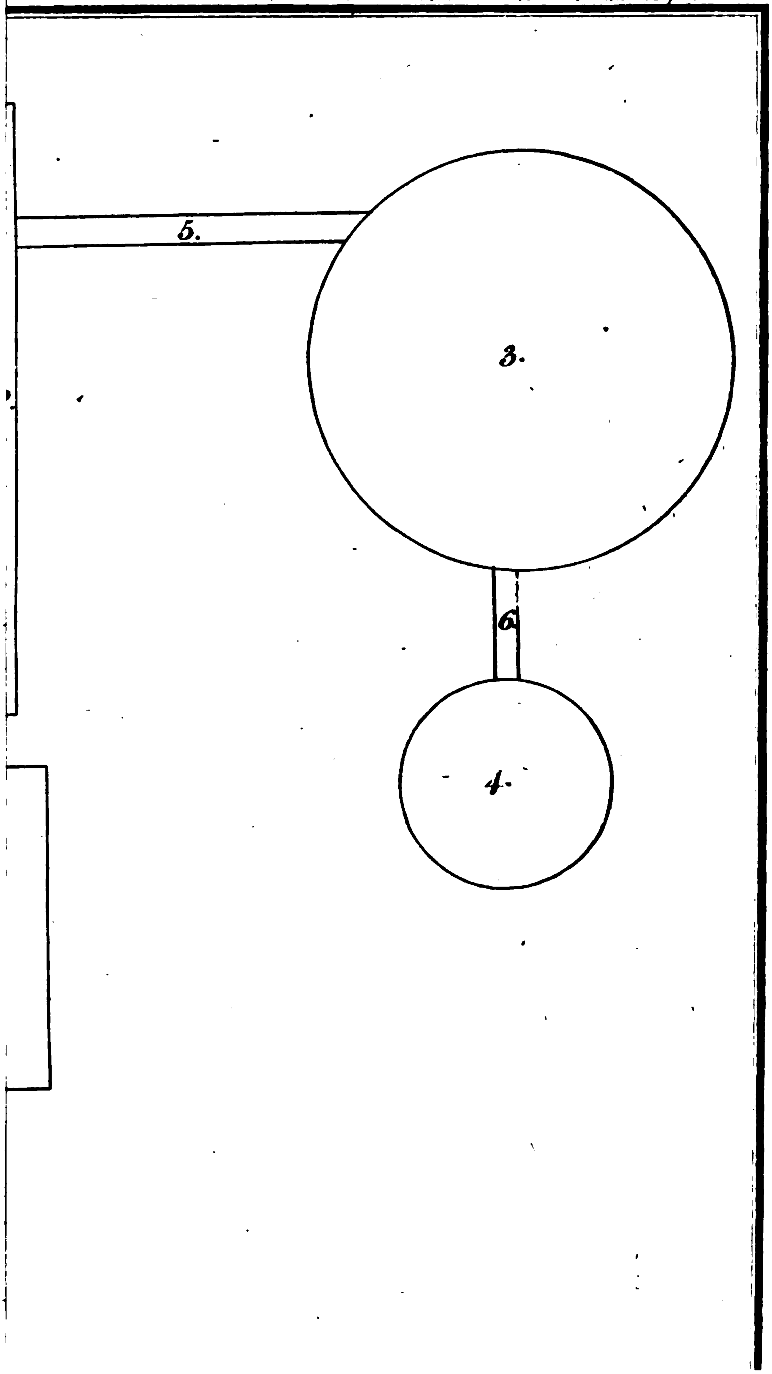
I have not leiſure, at preſent, to proſecute theſe experiments farther, and ſhall therefore content myſelf with making a few general obſervations on the facts that have been advanced.

1. This pot-aſh is a true fixed vegetable alkali, and a product of putrefaction which has not, that I recollect, been noticed by the chymiſts. A very celebrated writer has even in expreſs terms aſſerted, that "all vegetables, "not excepting thoſe which in their natural ſtate fur-"niſh aſhes containing much fixed alkali, when burnt, "after their acid has been altered by a complete putre-"faction, leave aſhes intirely free from alkali[(b)].

2. The quantity of alkali contained in this pot-aſh may, with ſome probability, be eſtimated at one-third of its weight; whereas the white Muſcovy aſhes are ſaid to yield only one-eigth part[(c)]. Of its impurities ſulphur is the moſt injurious to its bleaching powers, and ſhould in the preparation of it be carefully ſeparated. A longer continued and more gentle calcination in a furnace ſupplied with a ſufficient current of air might, perhaps, anſwer this end. But the moſt effectual method would be to lixiviate the ſalts with pure water, after a moderate fuſion, and then to evaporate them ſlowly to dryneſs.

(*b*) MACQUER's Dictionary of Chemiſtry, article Alkali.

(*c*) HOME on Bleaching, p. 157.


5.
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It muſt, however, be remarked, that in thus freeing the pot-aſh from phlogiſtic matter, another impurity is generated: for both the action of fire, and the ſolution in water, convert into earth a portion of the alkaline ſalt.

3. No quick-lime appears to be contained in this pot-aſh: for a ſolution of it, poured from its ſediment, remained clear, though long expoſed to the air. Nor did it acquire any milkineſs by being blown into from the lungs; but perhaps the addition of this cauſtic ſubſtance would increaſe its activity and value, when employed in ſoap-boiling and other arts: for the Ruſſian pot-aſh is more pungent to the taſte, ſaturates a larger proportion of acid, and diſſolves oils more powerfully than the pure alkaline ſalts. And Dr. HOME has proved[(d)], that theſe qualities depend on a large admixture of quick-lime.

4. It would be worthy of trial, to aſcertain whether the large purple ſediment, which ſubſides when this pot-aſh is lixiviated, might not be applied to the manufacture of Pruſſian blue; or uſed in the manner recommended by Mr. MACQUER for dying wool and ſilk. See the Memoirs of the French Academy for the year 1749[(e)].

5. The farmer, though he live at a diſtance from the manufactures in which pot-aſh is employed, may yet

(d) Eſſay on Bleaching. NEUMANN's Chemiſtry, by LEWIS.

(e) See alſo NEUMAN's Chemiſtry, by LEWIS, p. 73.

find

find his account in preparing it from dung-hill water: for it will furnifh him with a top dreffing for his garden and land, of great fertilizing powers. But if fuel be dear where he refides, and conveniences for combuftion be wanting, the fimple evaporation of the water may fuffice, and the putrid lye, thus reduced to a folid form, will prove to be a rich manure. At Hart Hill, my fummer abode, about three miles from Manchefter, I have lately practifed a method of making a compoft of the dung-hill water. The weeds and rakings of the garden, the dreffings of the fields, the leaves blown from the trees, and other refufe matters, are put together near the refervoir, out of which the water is occafioally pumped and fcattered over the heap: fo ftrong a ferment almoft inftantly excites putrefaction. And thefe vegetable fubftances are foon converted into a fertile mould, which retaining the falts and oils of the dung-hill water, fuffers the fuperfluous moifture to exhale into the air, or to percolate through it. And I have found, by experience, that the compoft thus prepared is laid on the meadows at lefs expence, and that it is more efficacious and durable in its operation than the fprinklings, which, at ftated times, they formerly received: for my land, though good and in fine condition, is light and

 fandy,

ſandy, and the dung-hill water quickly paſſed below the roots of the vegetables which grow upon its ſurface.

Mancheſter, March 3, 1780.

EXPLANATION OF THE FIGURE.

1. The dung-hill.

2. A ſough or drain round the dung-hill.

3. A reſervoir for the dung-hill water.

4. A well, communicating with the reſervoir, in which a pump is fixed, to convey it to N° 7.

7. A pan in which the water is boiled to the conſiſtence of treacle, and afterwards burnt in an oven. The pan is made of iron plates, turned up a little at the edges. To theſe are ſcrewed planks of wood, to make it about twenty inches deep.

XXIII. *On the Degree of Salubrity of the common Air at Sea, compared with that of the Sea-ſhore, and that of Places far removed from the Sea. In a Letter from* John Ingen Houſz, *M. D. F. R. S. to Sir* John Pringle, *Bart. F. R. S. Dated* Paris, Jan. 22, 1780.

Read April 24, 1780.

SIR,

AS you had recommended to me the examination of the air at ſea by the nitrous teſt, I followed your advice in my return to the Continent in the beginning of November laſt: and I embraced that opportunity with the more eagerneſs, as I knew that you had given credit to the account of ſeveral conſumptive people having recovered their health by going on ſea voyages, after the common means for curing that diſtemper had failed.

I was in hopes likewiſe to find in this inquiry, a confirmation of what you conjectured in your Anniverſary Diſcourſe in the year 1773, *viz.* that great bodies of water, ſuch as ſeas and lakes, are conducive to the health of animals, by purifying and cleanſing the air contaminated

nated by their breathing in it: ſo that the ſalutary gales, by which this infected air is conveyed to the waters, and by them returned again to the land, though they do riſe now and then to ſtorms and hurricanes, muſt nevertheleſs induce us to trace and to revere in them the ways of a beneficent Being, who, not fortuitouſly, but with deſign, not in wrath, but in mercy, thus ſhakes the waters and the air together, to bury in the deep thoſe peſtilential effluvia which the vegetables upon the face of the earth are inſufficient to conſume.

I was not without hope, that ſuch experiments might tend to throw a new light upon the cauſe of that almoſt univerſal effect of the ſea air, to wit, its increaſing the powers of life, and giving a keener appetite by haſtening the digeſtion of food.

I ſhall now give you an account of the experiments I made in conſequence of your ſuggeſtion, in the ſame order as they were made; and beg you to preſent them to the Royal Society, if you think them worthy the attention of that learned body.

I muſt firſt, however, premiſe, that as I wrote this paper in noiſy inns, on ſhip-board, and places little adapted to philoſophical application, I hope you will make ſome allowance for the inaccuracies which you may

 find

find in it. I began my experiments at Gravesend, where I was obliged to wait two days for a favourable wind. I found the air of that place, on Nov. 1, of a tolerable good quality, as one measure of it with one of nitrous air occupied, in several trials, about 104, or one measure and $\frac{4}{100}$ of a measure; so that I took it to be nearly of the same quality as the air of London.

The ship in which I went from London to Ostend happened to be becalmed about two or three miles from shore, in the mouth of the Thames, between Sheerness and Margate. The weather was very agreeable, warm, and the sun shone very bright, on the 3d of November. I was provided with a travelling apparatus, made on purpose by Mr. MARTIN, the whole of which was packed up in a box about ten inches long, five broad, and three and an half high. The glass tube or great measure, which was sixteen inches long, and divided in two separate pieces, lay in a small compass, and could be put together by brass screws adapted to the divided extremities. Instead of a water trough, such as is used commonly, I made use of a small round wooden tub, which I found on board the ship, and which I filled with sea-water, fixing to the edge of this tub, by means of a screw, the brass funnel, through which the air was to be let up into the glass tube.

After

After having exercifed myfelf during fome time in performing the experiment in a water tub much too fmall for the purpofe, I at laft acquired a habit of doing it tolerably well. I then began to make my experiments regularly at about eleven o'clock; and I have the pleafure to inform you, that I found the fea air at the place indicated of a fuperior purity to any common air I ever met with fince the month of June laft (when I began to engage in the courfe of experiments which have afforded me the materials of my work lately publifhed upon *Vegetables)* either in my country retirement, or in London. In fix different trials made one after another in the fhort manner defcribed in my book, p. 278. *et feq.* [a] I found, that the two meafures of air (one of common and one of nitrous air) occupied from 0.91 to 0,94; which difference in the refult, though but fmall in itfelf, was owing to the difadvantage of not having a veffel deep enough to move the glafs tube in with eafe, for the purpofe of mixing the two airs together, and to my not having yet acquired the habit of ufing the portable apparatus.

I tried alfo the air of this fpot in the manner ufed by Abbé FONTANA, which I have defcribed in my book,

(a) It confifts in letting up into the glafs tube one meafure of common air, and after this one meafure of nitrous air, and fhaking the tube forcibly in the water trough juft at the moment the two airs come into contact with each other.

p. 155 (b); the result of which trial was, that after the first measure of nitrous air was let up, the column of both airs occupied 1.86, or one measure and $\frac{86}{100}$ of a measure; after the second measure of nitrous air was let up, the bulk of both airs occupied 2.02, or two measures and $\frac{2}{100}$ of a measure: and after the third measure of nitrous air was let up, the remaining bulk of both airs occupied 2.96, or $\frac{96}{100}$ of a measure: so that the remaining bulk of both airs employed in the experiment (viz. two measures of common and three of nitrous air, each making 100 sub-divisions of the glass tube) amounted to two measures and $\frac{96}{100}$ of a third measure, or to 296 sub-divisions, which being subtracted from the five measures, or from the 500 sub-divisions employed, the remainder was 2.04, which was exactly the quantity of both airs destroyed.

Give me leave, before I proceed farther, to recall to your memory some of the experiments relative to this subject, made in my country retreat in the course of last summer, and related in my book, principally in p. 155. and 282. together with some further ones I made just

(b) It consists in letting up into the divided glass tube two measures of common air, and afterwards three measures of nitrous air, one after another, shaking the tube in the water trough after each measure of nitrous air, and beginning constantly this motion exactly at the moment the two airs come into contact with each other, or even before they meet, which is still better.

4 before

before my setting out for the continent: this recapitulation will make the nature of the experiments just mentioned, and their result, much more easily understood.

The different degrees of salubrity of the atmosphere, as I found it in general in my country house at Southall Green, ten miles from London, from June to September, lay between 103 and 109, that is to say, that of the two measures of air, *viz.* one of common and one of nitrous air, the remaining bulk or column occupied between 103 or 109 sub-divisions in the glass tube.

I was somewhat surprized, when upon my return to town to my former lodgings in Pall Mall Court, I found the common air purer in general in October than I used to find it in the middle of summer in the country; for on the 22d of October, at nine o'clock in the morning, the weather being fair and frosty, I found, that one measure of common air and one of nitrous air occupied 100 sub-divisions in the glass tube, or exactly one measure. It gave by the Abbé FONTANA's method above mentioned the following result, 184, 208, 304; so that the quantity of both airs destroyed in this trial was exactly one measure and $\frac{96}{100}$ of a measure, or 196 sub-divisions. That very day, at two o'clock in the afternoon (it being then rainy weather) the air was somewhat altered for the worse;

worſe; for at that time one meaſure of common and one of nitrous air occupied 102.

The next day, October 23, it being rainy weather, the ſtate of the atmoſphere was the ſame in the morning as it was the day before in the afternoon: for one meaſure of common and one of nitrous air occupied 102; and the reſult of Abbé FONTANA's method was as follows, $184\frac{1}{2}$, 211, 307, ſo that the quantity of both airs deſtroyed amounted to 193.

October the 24th, I again examined the ſtate of the atmoſphere at nine o'clock in the morning, the weather being ſerene; I found it reſtored to its former goodneſs; for one meaſure of common and one of nitrous air occupied 100, or exactly one meaſure; and the reſult of Abbé FONTANA's method was 184, 207, 304. At ſeven o'clock in the evening of the ſame day the air was again grown worſe; for one meaſure of common and one of nitrous air occupied 103.

October 25th, I examined the air at eleven o'clock in the morning, the ſky being cloudy, and found, that one meaſure of common and one of nitrous air occupied 102. I put it again to the teſt at eleven o'clock at night, and found, from five different trials, that one meaſure of it with one of nitrous air occupied 105.

October 26th, the weather being very dark and rainy, I found that one meaſure of common and one of nitrous air occupied 105.

You know, SIR, that the ſituation of my lodgings was ſuch, that the back part of the houſe was contiguous to the garden of Carleton Houſe, which is amply furniſhed with lofty elm trees, ſo as to make the garden appear from my windows like a foreſt. As I had diſcovered, in the courſe of laſt ſummer, the great power which the leaves of trees poſſeſs of improving the atmoſphere, by pouring down an inviſible ſhower of purified or dephlogiſticated air, during the day-time, in clear weather; I could not forbear aſcribing in ſome meaſure the purity of the common air of that ſpot to the happy ſituation of the place juſt by ſo many trees, which had all kept their leaves in full vigour till that time. But I only give this as a conjecture; for I am really ſorry that I cannot prove it by a direct experiment. To put out of all controverſy ſuch a powerful influence of neighbouring trees upon the circumambient and unconfined air[(c)], the air of different and diſtant

(c) I ſay *unconfined*, for I am not quite ſure that the influence of trees and other vegetables, though in reality very great (as I think I have put beyond all doubt in my book) can be more ſenſible near trees in the open air than at ſome diſtance; in the ſame manner as ſome diſtilled water, poured gradually among an immenſe maſs of common water, would diffuſe itſelf ſoon through the whole maſs equally.

places

places ſhould be examined at the ſame time as the air near the trees. I was deſirous of doing it, and gave orders to my ſervant to gather air from different parts of London; but the hurry of buſineſs, occaſioned by our approaching departure, made him forget or overlook my orders; and for the ſame reaſon I myſelf forgot to perform the taſk. After all, the beſt, and perhaps only good way to clear up this matter would be, if ſeveral philoſophers, each provided with an exactly ſimilar apparatus, and living in different parts of the town, and in different parts of the country, ſhould at the ſame time put the air of the place of their abode to the teſt, and afterwards compare their notes. I conceived ſome flattering hopes before I ſet out from London, that ere long ſuch eudiometers, as I have deſcribed in my book, will come into the hands of many good philoſophers who will take upon them with pleaſure to perform this taſk, as I heard that ſome of them were already ordered to be made for many perſons, among the reſt for Dr. HEBERDEN.

Though I have ſome reaſon to believe, that the vicinity of the trees did really contribute ſomewhat to render the air purer at the above mentioned place; yet I believe alſo, that the froſty weather of itſelf contributes to purify the atmoſphere (perhaps by checking a great

many

many caufes of corruption of various fubftances); and that the livelinefs we commonly enjoy in frofty weather is in a great meafure owing to the fuperior degree of purity of our common element at that time.

I muft now return from this digreffion to the nature of the fea air.

From what I have already related it appears, that the difference between the beft atmofpheric air I have yet found and the fea air, as I found it by the firft examination upon the fpot where chance carried me, is as 91 to 100, the leffer number indicating the beft quality. Now, as I found the fea air of fuch a pure quality fo near land, I thought it might, with fome degree of probability, be expected, that the common air, at a diftance from land, would prove of a ftill fuperior quality; for I could fcarce believe, that in the firft trial, made without choice of place or time, I had juft hit upon a time and a place where the fea air is of the firft quality.

I would have repeated the fame experiment next day, November the 4th, when we were in the middle of the channel between the Englifh coaft and Oftend; but the motion of the fhip, which was very great, made it impracticable. Not intirely, however, to lofe the opportunity which the voyage afforded me, I filled three phials, made on purpofe for fuch ufe, with

air, when we were in the middle of the ſea (the wind blowing pretty hard, and the weather being rainy). I kept theſe bottles ſhut till next day, November 5th, when I examined the air confined in them at Oſtend, and found it of an inferior quality to that which I had tried on the 3d of November, in the mouth of the Thames; for one meaſure of it with one of nitrous air occupied, in three different trials, 097. I found the common air at Oſtend near as good the ſame day at ten o'clock in the morning (the weather being cloudy); for one meaſure of it with one of nitrous air occupied 098. In the afternoon (the weather being very rainy) the common air of the place was become worſe, though ſtill of a very good quality; for one meaſure of it with one of nitrous air occupied 100, or exactly one meaſure.

About five o'clock in the evening, the weather continuing rainy, and the wind blowing at that time very hard from the ſea, I went to the ſea-ſhore on purpoſe to gather in a phial, fitted for the purpoſe, the ſea air juſt as it blew towards the land. When I had got it I went immediately home, and put it directly to the nitrous teſt. I found, by ſeveral trials of ſuch good quality, that it was nearly as good as that which I had met with in the mouth of the Thames; for one meaſure of it with one of nitrous air occupied 094 and 095, whereas

whereas the common air of the inn (as I found by trial at the time) was somewhat of an inferior quality, though still remarkably pure; for one measure of it with one of nitrous occupied 0,97 in five repeated trials.

As the difference in the quality of the sea air examined on the spot in the mouth of the Thames, November 3d, and that which I gathered in the middle of the sea in rainy and windy weather, was so remarkable, I suspected the reason of this difference to be, that the air, put to the test November 3d, had been exposed during several days to the influence of the sea without any mixture of land air, as it had been remarkably calm all that time; and that the air gathered on the sea in windy weather was mixed with air driven from the land by the wind, and incorporated with the sea air. This suspicion was afterwards strengthened when I found the air gathered at the sea shore, on the evening of November 5th, near as good as that which I gathered on a fair day in the mouth of the Thames, November 3d; for the wind being N.W. the air driven upon the coast was to be considered as true sea air, without any mixture of land air.

But after I had made up my mind about the difference of the above related experiments, a doubt rose in me about a circumstance to which this difference might have been owing, at least in some measure; the circumstance I mean

was this: I had made ufe of fea water for the experiment on the 3d of November, whereas I had made ufe of pump water in examining the fea air kept in bottles at Oftend. I thought I had no right to draw any conclufion from the fact till I was convinced that the making this experiment in common or in fea water would make no difference in the refult. This confideration made me ftay one day longer at Oftend, on purpofe to fatisfy my mind on this head, left I fhould never find another opportunity of doing it. I immediately ordered a pail full of fea water to be brought to my lodging, and made feveral comparative trials with atmofpheric air in common water and in this fea water; but I could not obferve any real difference in the refult. Thus all degree of fufpicion about the difference of the refult from this caufe was now at an end.

There now only remained fome little fufpicion that the air gathered on the fea, and kept in phials, might have undergone fome alteration; this might have been the cafe, as I had found in fome former experiments made in England, that air kept in bottles was fometimes liable to alterations, which I think is partly to be afcribed to the difficulty of finding bottles fo well fecured by a ground ftopper as to fhut out every communication with the external air, and partly, perhaps, to the nature of air, which is not in itfelf an unalterable fubftance,

as

as I have attempted to prove in my book upon Vegetables, p. 107: in the preſent caſe, however, I rather incline to think, that the reaſon why the air, gathered in rainy and windy weather on the ſea, was found of an inferior quality, is to be aſcribed chiefly to the land air being driven by the wind upon the ſea, and thus to the mixture of both airs.

November the 6th, about nine o'clock in the morning, the weather being cold, windy, and cloudy, I again put the common air of Oſtend, which I gathered in my inn, to the teſt, and found it of near as good a quality as that in the mouth of the Thames; for one meaſure of it with one of nitrous air occupied 094½ in three repeated trials.

The ſame morning, about eleven o'clock, the wind blowing very hard from the ſea, I went to the ſhore on purpoſe to gather ſome air juſt as it came from the ſea. I found its quality inferior to that which I had examined two hours before, though ſtill ſuperior to any air I have yet found in England; for one meaſure of it with one of nitrous air occupied 097. The common air, as I found it in my lodging, was at 098. The wind had not ſhifted much, though I cannot aſcertain the exact point from which it then blew. It ſeems probable from the foregoing experiments, that though in general the ſea air

ſurpaſſes the land air in purity, yet there are the ſame inconſtancies in its degree of goodneſs as in the land air. I will not attempt to give any reaſon for this inconſtancy, as I have no experiments to ſupport any; but I think it highly deſerving the attention of philoſophers to attempt the diſcovery of this phenomenon.

This experiment being finiſhed, I cloſed my inquiries at Oſtend, and ſet off for Bruges.

We arrived at dark, and about ſeven o'clock in the evening I tried the common air of that place, and found it inferior in purity to that of Oſtend in above ten experiments: one meaſure of it with one of nitrous air occupied 105, or thereabout. I had the mortification to find the ſtoppers of the phials in which I had kept the air of Oſtend all looſened, ſo that I could not make any comparative trial with both airs.

November 8th, I ſet out for Ghent, where I ſpent the next day, November 9th. I tried the air of that place about three in the afternoon, and found it better than that of Bruges; for one meaſure of it with one of nitrous air occupied 103, or thereabout, in ſeveral experiments.

As all the following trials with common air are made in the ſame way as the foregoing, *viz.* by mixing one meaſure of common with one of nitrous air, I will hereafter only

only mention the numbers of the reſult, for the ſake of abridging the paper by avoiding continual repetitions.

November 12. At Bruſſels, I found the air, at ſeven o'clock in the evening, at $105\frac{1}{2}$.

November 13. I found the air of the lower part of the city at 106, that of the higheſt part at 104; the weather was rainy and damp. It is a common opinion at Bruſſels, that the air in the lower parts of the city is more unwholeſome than in the higher parts, and that people of bad conſtitutions cannot ſtand it unleſs they go to live in the higher parts.

November 14. I found the air of the lower and higher parts of the city of the ſame goodneſs, each being at 103. The weather was fair and froſty.

November 15. The air was the ſame as yeſterday in both parts of the city; the weather fair and cold.

November 22. I arrived at Antwerp, where I found the air in the evening at $109\frac{1}{2}$; the weather was rainy, damp, and cold.

November 23. I ſet out for Breda. I filled a phial with air, when I ſet out from Antwerp at eight o'clock in the morning. I alſo filled a phial with air on the middle of the heath or common called *De Lange Hey:* the weather was remarkably cloſe and damp. I tried theſe two airs at night, at Breda; that of Antwerp was found

at 106; that of Breda the fame; that of the heath at $105\frac{1}{2}$.

November 24. I examined the air at Breda in the morning, about eleven o'clock, the weather being fair, cold, and inclining to froft; it was at 102. At feven o'clock in the evening it was at 103.

November 25. The air at Breda was at 104; the weather rainy and cold.

November 26. The air was in the morning and in the evening 103; the weather very rainy, cold, and ftormy.

November 27. I fet out from Breda for Rotterdam, and croffed the water at the Moordyke. I tried the air at the Moordyke clofe to the water, and found it at $101\frac{1}{2}$; the weather was fair and cold, but not frofty. This fpot is reckoned very healthy; the inhabitants have a found look, and live to a great age in general.

November 28. I examined the air at Rotterdam, it was at 103; the weather rainy and cold.

November 29. Being at Delpht, I gathered fome air in the middle of the day, the weather being ftormy and rainy. I examined it next day at the Hague, and found it at 103; and that of the Hague 104.

November 30. I examined the air at the Hague, and found it at 104; the weather cold, the wind Northerly.

December

December 1. Being ſtill at the Hague, the weather underwent a ſudden and remarkable change. The wind was ſoutherly and ſtormy; the air was become ſo warm that upon going out of the houſe into the ſtreet, I felt the ſame ſenſation as upon coming from a cold air into a room heated by a German ſtove. I ſuſpected that this ſudden change would alter the conſtitution of the atmoſphere in point of ſalubrity. Having no time to make any experiment, I contented myſelf with filling ſome phials with this air, and ſending my ſervant to Schevelingen to gather ſome air cloſe to the ſea.

December 2. The wind and weather remained the ſame as yeſterday. In the evening, about eight o'clock, when FAHRENHEIT's thermometer ſtood at 54°, I put the common air to the nitrous teſt, and found it at 116; the air gathered the day before at 117; and that gathered cloſe to the ſea at 115. As I had never found the common air near ſo bad, I had ſome apprehenſion that my eudiometer was out of order, or that ſomething was the matter with the nitrous air. I made therefore freſh nitrous air, and repeated the experiment many times, but the reſult was nearly the ſame. In the mean time, I had the following accidental meeting. The father of the landlady of the houſe having been informed by the ſervant, that I was about ſome extraordinary purſuit, of which

he could have no conception, was led to come and see what I did. He had ſcarce been a minute with me but I perceived he laboured under a ſevere aſthma. He explained his caſe to me, knowing me to be a phyſician, and told me, *that he had paſſed theſe two days very uncomfortably, finding the air ſo uncommonly heavy that he could ſcarce draw his breath:* which convinced me, that the element was in reality become of an inferior quality.

December 4. Being at Amſterdam, I found the air of that place at 103; the weather being rainy, windy, and cold.

December 5. The air was at 102; the weather nearly as yeſterday.

December 10. I returned to Rotterdam, and found the air at 101; the weather rainy.

In the beginning of laſt year they made an end of draining a large meer (about half the ſize of the Haerlemmer Meer) ſituated in the neighbourhood of Rotterdam, which was turfed out in former ages. It was now laid into arable land, and turned out to be very fruitful. When this land was quite cleared of the water, an uncommon epidemical diſeaſe broke out in all the places ſituated upon the borders of this lake; it began about Auguſt, and abated when the winter ſeaſon ſet in. This diſtemper broke out a-freſh laſt ſummer, and was

now again upon its decline. It carried off a great number of inhabitants. It appeared chiefly under the habit of an irregular intermittent, a bilious remitting, and a putrid fever. There was ſcarce a ſingle houſe to be found in which there were not ſome perſons ſick. The villages at a quarter of a league diſtance from the former lake were free from it. This diſtemper was aſcribed to the putrid exhalations of this newly uncovered land; which exhalations were very offenſive to the ſmell. This was ſo much the more probable, as the diſeaſe abated when the ſtench, checked by the cold, abated. I tried the air of this former lake upon the ſpot, in company with my learned friend Dr. DE MONCHY, profeſſor of phyſic, and found it as good as that of Rotterdam; but there was a great deal of wind that day, and no perceptible ſtench. However, Dr. BICKER, an eminent phyſician of that city, got me a phial filled with air of this lake, which he took from a ſpot where he ſtill perceived ſome of the former bad ſmell. This air proved to be in reality of an inferior quality to that of the city.

December 12. Being in the middle of the water between Dort and the Moordyke, I found the air upon what is called Holland's Diep of an inferior quality, the weather being remarkably dark, rainy, and windy; it was at 109.

December 13. Being returned to Breda I found the air of that place at 109 in the morning, the weather continuing as it was yefterday. In the afternoon it was fomewhat better, *viz.* at 106½, the weather having cleared up.

December 16. Having returned to Antwerp, I found the air of the lower part of that city at 105; and that of the higher part at 104, the weather being rainy and temperate.

December 17. The air at Antwerp was 107, the weather continuing to be nearly as it was the day before.

December 19. Being returned to Bruffels, I found the air at 109, the weather being rainy, windy, and rather warm.

December 21. I found the air at Bruffels at 106, the weather being dry and cold.

December 22. The air of Bruffels was the fame as yefterday; the weather was nearly the fame alfo.

December 23. I arrived at Mons, and found the air of that place at 104; the weather rainy and cold.

December 24. Being near Bouchain, I found the air at 104½; the weather was cloudy and cold.

December 25. I tried the air at Peronne, and found it at 102½; the weather was frofty.

December

December 26. Being at Cuvilli, a village four leagues from Roye, I examined the air of that place, and that which I had gathered on the road about twelve o'clock in the day-time; I found them both at 103; the weather was frosty.

December 27. I examined the air at Senlis, and that which I had gathered in the middle of the day on the road; both were at $102\frac{1}{2}$; the weather continued frosty.

December 29. Being at Paris, I found the air in that capital at 103; the weather frosty.

1780, January 8, the air of Paris was at 100; the weather very frosty.

January 13. The air of Paris was 98; it froze very hard.

This paper being already too long, I will only add some few deductions, which seem to follow of themselves from the above related experiments.

It appears from these experiments, that the air at sea and close to it is in general purer and fitter for animal life than the air on the land, though it seems to be subject to the same inconstancy in its degree of purity with that of the land; so that we may now with more confidence send our patients, labouring under consumptive disorders, to the sea, or at least to places situated close to the sea, which have no marshes in their neighbourhood.

It

It seems also probable, that the air will be found in general much purer far from the land than near the shore, the former being never subject to be mixed with land air.

It appears also, that the air in frosty weather is in general wholesomer than it is in winter when it does not freeze; and that uncommon warm weather, happening in the winter season, is apt to render the atmosphere very unwholesome: the reason of which I apprehend to be, that the frost totally checks that general tendency to corruption, which being revived by warmth again increases the infection of the common air, which at that time is so much the greater, because the plants (which are deprived of their leaves in winter) have no power in them to counteract it.

It seems also probable, that those countries which are, by their local situation, exposed to noxious exhalations, are in general much wholesomer in the winter; and that it is much safer to cross such countries in summer time when it is windy weather than in a calm, &c.

How far these deductions are founded upon experience may appear by applying them to such places as they may be found to have a relation to.

My old friend Dr. DAMMAN, an eminent physician and professor royal in midwifry at Ghent, told me, that when

when he was formerly a practitioner at Oſtend, during ſeven years, he found the people there remarkably healthy; that nothing was rarer there than to ſee a patient labouring under a conſumption or aſthma, a malignant, putrid, or ſpotted fever; that the diſeaſe to which they are the moſt ſubject, is a regular intermittent fever in autumn, when ſudden tranſitions from hot to cold weather happen.

People are in general very healthy at Gibraltar, though there are very few trees near that place; which, I think, is owing to the purity of the air, ariſing from the neighbourhood of the ſea.

Moſt ſmall iſlands are very healthy.

At Malta people are little ſubject to diſeaſes, and live to a very advanced age.

XXIV. *The principal Properties of the Engine for turning Ovals in Wood or Metal, and of the Instrument for drawing Ovals upon Paper, demonstrated. By the Rev. Mr.* Ludlam, *Vicar of* Norton, *near* Leicester; *communicated by the Astronomer Royal.*

Read May 4, 1780.

THE instrument for drawing ovals upon paper or board is so common, that a particular description of it is needless. It is much in use among the joiners, and called by them *the trammels*. One part of it consists of a cross with two grooves at right angles: the other is a beam carrying two pins which slide in those grooves, and also the describing pencil; we shall distinguish these two parts by the names of the *cross* and the *beam*.

It is very well known, that all the engines for turning ovals are constructed on the same principles with the trammels; the only difference is, that in the trammels the board is at rest, and the pencil moves upon it; in the turning engine, the tool (which supplies the place of the pencil) is at rest, and the board moves against it.

Let

Let Aa and Bb (fig. 1.) be two indefinite lines, interfecting each other at right angles in C. Let LSM be the beam, or a rigid right line, in which affume two fixed points L and S at pleafure. If the fixed point L be kept always fliding upon the line BCb, and the other point S always fliding upon the line ACa; I fay then, that any point M in the line LS, or that line produced, will defcribe an ellipfe.

Bifect LS in E, and through C and E draw the indefinite right line CEH. Upon LS as a diameter with the center E defcribe a femi-circle, and becaufe LCS is a right angle, it will pafs through C, and EC=EL. Through M draw MPH perpendicular to AC meeting CE produced in H; and becaufe MH is parallel to CL, the triangles MEH and CEL are fimilar, and HE=ME, and HE+EC=ME+EL, or CH=LM. The point H therefore always falls in the circumference of the circle HADa defcribed with the center C and radius CH=LM. Now the fimilar triangles CHP and SMP give CH : SM :: PH : PM. But when L arrives at C, then LM (=CH) coincides with CA; and when S arrives at C, then SM coincides with CB; therefore CA : CB :: PH : PM, and $CA^2 : CB^2 :: PH^2 : PM^2$, or $CA^2 : CB^2 :: AP \times Pa : PM^2$, which is the property of an ellipfe, whofe firft femi-axe is CA or LM, and fecond femi-axe is CB=SM.

 Produce

Produce PM till it meets the circle in N, and draw the radius CN; then PH=PN and CA : CB :: PN : PM. Again, becaufe PCH=PCN, therefore NCD=ECL=ELC and CN is parallel to LM, and CL=NM. Draw M*p* perpendicular to B*b*, cutting CN in *n*, and for the like reafon C*n*=SM=CB, and CS=M*n*. While the point M defcribes an oval, the point E defcribes a circle whofe center is C and radius CE=$\frac{1}{2}$SL.

To the ruler MEL (fig. 2. and 3.) fix another ruler or right line *m*EK paffing through E, fo that the ruler *m*EK may be carried about by the ruler MEL, keeping the angle ME*m* between the two rulers invariable. On *m*EK take EV=EK, and each = ES or EL, I fay, the point V will defcribe a right line *a*VC*α* paffing through C, and making an angle *a*CS with CA, equal to half ME*m* the angle made by the two rulers; the point K will alfo defcribe a right line *b*KC*β* paffing through C, and making an angle *b*CL, with CL, alfo equal to half ME*m*.

On the center E (fig. 2. and 3.) and with the radius EC defcribe a circle and it will pafs through the points S, V, C, L, K; draw the lines VC and KC, and the angles SEV, and SCV, both ftand on the fame arch SV; the former at the center E, the latter at the circumference C; therefore the former is double the latter. In like manner the angles KEL and KCL both ftand on the fame arch KL,

the former at the center, the latter at the circumference; therefore the former is double the latter. Now as this holds in every poſition of the rulers during their joint motion, it is manifeſt, the points V and K will each deſcribe right lines, namely, $ac\alpha$ and $bc\beta$, paſſing through C and making the angles aCA and bCL (=BCβ) each equal to half MEm.

Hence the lines $ac\alpha$ and $bc\beta$, traced by the points V and K are at right angles, and the ruler mVEK moves exactly in the ſame manner as if it was guided by the the points V and K ſliding on the lines $ac\alpha$ and $bc\beta$, at right angles to each other; juſt in the ſame manner as the ruler MSL is guided by the points S and L, ſliding on the lines AC and BC. Therefore if any point m be aſſumed in the line KVm as a deſcribing point, the figure deſcribed will be an ellipſe, the poſition of whoſe principal axes are the lines $ac\alpha$ and $bc\beta$; the center of the ellipſe being ſtill in C as before. If mK is taken equal to ML, the ellipſe thus deſcribed by the point m will be the ſame with that deſcribed by the point M, only in another poſition: its greater ſemi-axis aC making an angle with AC, the greater ſemi-axis of the former ellipſe, equal to half MEm, the angle which the rulers or lines ME and mE make with each other.

Scholium. This propofition is demonftrated in SCHOOTEN's *Exercitationes*, &c. p. 305.; but he makes twelve cafes of it: had he made ufe of the 20th of the 3d El. they might have been all comprehended under one.

In the turning of ovals, the top of the *reft* which fupports the tool is always made to pafs through S and L (fig. 1.) the two centers round which the oval engine turns; and in this cafe the ruler or line MSEL reprefents the top of the *reft*. If the tool be held on any part of the *reft* between the work-man, and the neareft center as at M, an oval will be turned having its longer axis A*a* (in one pofition of the work) coinciding with the top of the *reft*. As the tool is removed towards S, the oval will grow narrower and at S become a right line. Beyond S towards E it will grow rounder, and at E become a circle; beyond E it will grow narrower, and at L become a right line at right angles to the right line defcribed when the tool was at S. If the tool be removed beyond L, it will defcribe an oval again, whofe longer axis is at right angles to the longer axis of the oval firft defcribed when the tool was at M. It may be very convenient to mark the points S and L and alfo their middle point E on the top or face of the *reft* that fupports the tool. If any thing be interpofed between the tool and the top of the *reft* fo as to raife the tool above the line paffing through the centers

S and

S and L, an oval will yet be defcribed, whofe center will be the fame with that of the oval firft defcribed when the tool was at M; but its principal axis will crofs the principal axis of that oval (fig. 2. and 3.). Draw right lines both from M the old place of the tool, and from *m* the new place of the tool, to the point E marked on the *reft*. Half the angle which thefe two lines make with each other will be the angle which the principal axis of the new oval makes with the principal axis of the old one.

It is well known, that when the oval engine is fet in order for working, there is a part which flides back, and is then fixed, which feparates the two centers of motion and gives the eccentricity; for the difference between the firft and fecond femi-axes will be juft as much as the centers are thus feparated: call the diftance between the two centers E; let now the tool be fixed in any place, upon, above, or below the *reft*; call *m* E the diftance of the tool from the middle point between the centers (marked E on the *reft)* D; and the greater femi-axis of the oval fo defcribed will be $D+\frac{1}{2}E$, and the leffer femi-axis $D-\frac{1}{2}E$; and thus both the form and pofition of the oval will be known. All workmen know the tool muft never be raifed above the place where it was at firft held, and we

fee the reafon; it would deftroy the oval firft begun to be turned, and form a new one in a different pofition.

But there is another difficulty in turning ovals, efpecially fuch as have mouldings, as picture-frames, &c. The tool generally has all the mouldings formed upon it: now if it be laid flat upon the *reft*, and the engine fet to work; the mouldings will in fome places crofs the plane of the tool (or the top of the *reft)* at right angles (as in turning circles), in other places obliquely. This will make the feveral members of the mouldings *leaner* or fmaller in one part of the work than another. Nor will the cafe be altered if the mouldings be turned feparately. Analogous to this, when an oval is drawn by the trammels, the line defcribed by the pencil will not, as in a circle, be always at right angles to the beam of the trammels. The oval line fo drawn will be at right angles to the defcribing beam, only at the extremity of the two principal axes where the beam coincides with thofe axes; in all other places the oval line and beam make an oblique angle. It may be proper therefore to enquire how much this angle deviates from a right angle. This we fhall call the angle of *deviation*.

All things as in fig. 1. draw the tangents TM and TN, to the point M in the ellipfe and the point N in the circle correfponding to each other; and from the nature of the ellipfe

ellipſe theſe tangents will meet each other in the axis CA produced. Draw MG perpendicular to TM, and GMS will be the angle of deviation ſought. I ſay, the angle MTN, between the tangents to correſponding points in the ellipſe and circumſcribing circle, is equal to the angle of deviation GMS.

For becauſe TNC is a right-angled triangle, and NP perpendicular to TC; therefore TNP=NCP=MSP, that is (in the triangles MTN and GMS) the angles TNM and MSG are equal. In like manner, becauſe TMG is a right-angled triangle and MP perpendicular to TG, therefore TMP= MGP, and (in the triangle MTN and GMS) the angles TMN and MGS are equal; therefore in the ſame triangles, the remaining angles MTN and GMS are alſo equal.

To compute the angle MTN, we have by trigonometry $TP^2+PM\times PN : TP :: MN :$ tan. MTN, radius being unity. Call now $CA=t$, $CB=c$, $CP=x$, $PM=y$, $CA-CB$ (or $t-c$) $=d$, and we have $PN=\sqrt{tt-xx}$; alſo $CD : CB :: PN : PM =\sqrt{tt-xx}\times\frac{c}{t}$, whence $PM\times PN=\overline{tt-xx}\times\frac{c}{t}$. Again $CP : PN :: PN : PT$, whence $TP=\frac{tt-xx}{x}$. Laſtly, $CD : BD :: PN : MN=\sqrt{tt-xx}\times\frac{d}{t}$; whence the tangent of MTN the angle ſought is $\frac{dx\sqrt{tt-xx}}{ttt-dxx}$: and this is a *maximum* when $\frac{ttt}{2t-d}$, or $\frac{ttt}{t+c}=xx$, or when $\frac{ccc}{t+c}=yy$, or when CP and PM have ſuch a proportion that $CP^2 : PM^2 :: CA^3 : CB^3$.

Let

Let AMBab (fig. 4.) be an ellipse whose center is C; draw the circumscribed and inscribed circles as before; the former cutting the second axis produced in D, the latter cutting the first axis in d, and the second axis in b. On Db as a diameter describe a circle cutting the first axis Aa in Q, draw DQ and bQ. Set off CR=DQ, join DR, and draw DP at right angles cutting the first axis in P, draw PM an ordinate to that axis, and M will be the point in the oval line where the angle of deviation is greatest. Otherwise, upon Cb produced set off Cr=bQ, join dr, and draw dp at right angles cutting the second axis in p: draw pM an ordinate to that axis, and M will be the point where the angle of deviation is greatest.

At the maximum (when $xx=\frac{ttt}{t+c}$) $PN^2=\frac{ttc}{t+c}$, $PM^2=\frac{ccc}{t+c}$, and $TP^2=\frac{tcc}{t+c}$: whence $TP^2=PM\times PN$. Also, PN, PM, TP, are to each other as CA, CB, and $\sqrt{CA\times CB}$, respectively. Therefore, $\frac{CA-CB}{\sqrt{Aa\times Bb}}$ is the tangent of MTN, radius being unity. Also $\sqrt{\frac{CA}{CB}}$ is the tangent of NTP; and MTP is the complement of NTP: therefore MTN is twice the excess of NTP, above 45°.

Fig. 4.

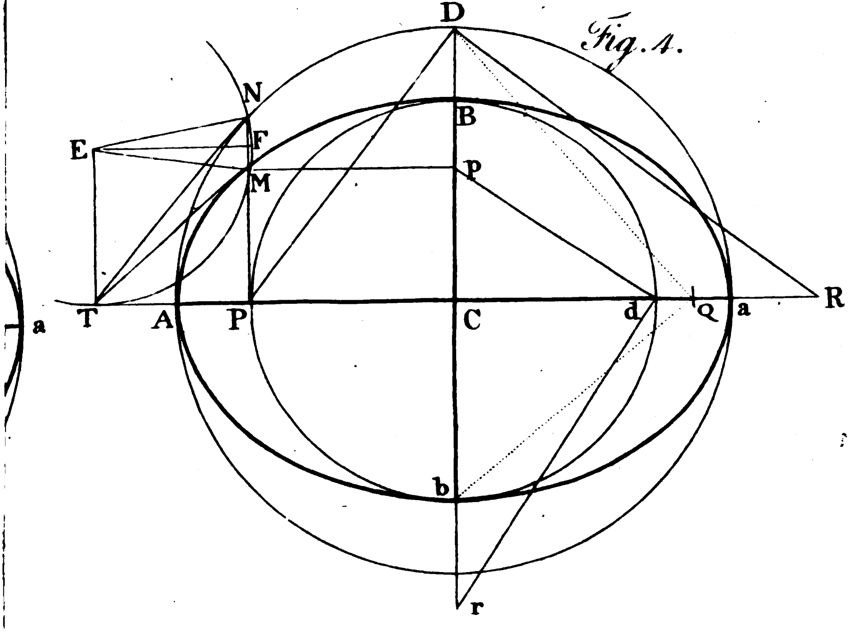
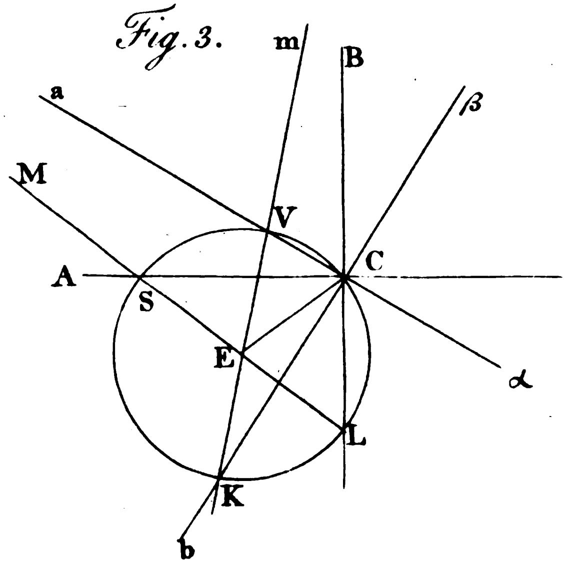


Fig. 3.



Basire Sc.

XXV. *Of Cubic Equations and Infinite Series.*

By Charles Hutton, *LL.D. F. R. S.*

Read June 1, 1780.

THE following pages are not to be underſtood as intended to contain a complete treatiſe on cubic equations, with all the methods of ſolution that have been delivered by other writers; but they are chiefly employed on the improvements of ſome properties that were before but partially known, with the diſcovery of ſeveral others which to me appear to be new and of no ſmall importance: for I have only ſlightly mentioned ſuch of the generally known properties as were neceſſary to the introduction or inveſtigation of the many curious conſequences herein deduced from them.

Art. 1. Every equation, whoſe terms are expreſſed in ſimple integral powers, has as many roots as there are dimenſions in the higheſt power. And when all the terms are brought to one ſide of the equation, and the

 coefficient

coefficient of the firft term or higheft power is $+1$, then the coefficient of the fecond term is equal to the fum of all the roots with contrary figns; the coefficient of the third term is equal to the fum of all the products made by multiplying every two of the roots together; the coefficient of the fourth term, to the fum of all the products arifing from the multiplication of every three of the roots together; &c. and the laft term, equal to the continual product of all the roots; the figns of all of them being fuppofed to be changed into the contrary figns before thefe multiplications are made. All this is evident from the generation of equations. And from thefe properties of the coefficients the following deductions are eafily made.

2. If the figns of all the roots of an equation be changed, and another equation be generated from the fame roots with the figns fo changed; the terms of this laft equation will have the fame coefficients as the former, only the figns of all the even terms will be changed, but not thofe of the odd terms: for the coefficients of the fecond, fourth, and the other even terms, are made up of products confifting each of an odd number of factors; while thofe of the third, fifth, and other odd terms are compofed of products having an even number of factors: and the change of the figns of all the factors

 produces

produces a change in the ſign of the continual product of an odd number of factors, but no change in the ſign of that of an even number of factors. Wherefore, changing the ſigns of all the even terms, namely, the ſecond, fourth, &c. produces no alteration in the roots, but only in their ſigns, the poſitive roots being changed into negative, and the negative into poſitive. But by changing any or all the ſigns of the odd terms, the equation will no longer have the ſame roots as before, but will have new roots of very different magnitudes from thoſe of the former, unleſs the ſign of the firſt term or higheſt power is changed alſo; but this term is always to be ſuppoſed to remain poſitive.

3. It alſo follows, that when any term is wanting in an equation, or the coefficient of any term equal to o, the ſum of the negative products in the coefficient of that term is equal to the ſum of the poſitive products in the ſame. And if it be the ſecond term which is wanting, then the equation has both negative and poſitive roots, and the ſum of the negative roots is equal to the ſum of the poſitive ones. But if it be the laſt term which is wanting, then one of the roots of the equation is equal to nothing. And hence ariſes a method of tranſforming any equation into another which ſhall want the ſecond term: and to this latter ſtate it will be proper to

transform every cubic equation before we attempt the refolution of it.

4. Let therefore $x^3 + px = q$ be fuch a cubic equation wanting the fecond term, where p and q reprefent any numbers, pofitive or negative.

5. Now from the premifes it follows, that this equation has three roots; that fome are pofitive, and others negative; that two of them are of one affection, and are together equal to the third of a contrary affection, namely, either two negative roots, which are together equal to the other pofitive, or two pofitive roots equal to the third negative.

6. But the figns of the three roots are eafily known from the fign of the quantity q; the fign of the greateft root being the fame with the fign of q when this quantity is on the right-hand fide of the equation, and the other two roots of the contrary fign. For when q is on the fame fide of the equation with the other terms, it has been obferved, that it is always equal to the continual product of all the roots with their figns changed; confequently q is equal to the product of all the roots under their own figns, when that quantity is on the other or right-hand fide of the equation: but the product of the two lefs roots is always pofitive, becaufe they are of the fame affection, either both + or both −; and therefore

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this product, drawn into the third or greateſt root, will generate another product equal to q, and of the ſame affection with this root.

7. But the roots of equations of the above form are not only poſitive, negative, or nothing, but ſometimes alſo imaginary. We have found that the greateſt root is poſitive when q is poſitive, and negative when it is negative; as alſo that one root is = to o when q is = o, and in this caſe the other two roots muſt be equal to each other, with contrary ſigns. But to diſcover the caſes in which the equation has imaginary roots, as well as many other properties of the equation, it will be proper to conſider the generation of it as follows.

8. The roots of equations becoming imaginary in pairs, the number of imaginary roots is always even; and therefore the cubic equation has either two imaginary roots, or none at all; and conſequently it has at leaſt one real root. Let that root be repreſented by r, which may be either poſitive or negative, and may be any one of the real roots, when none of them are imaginary: then ſince any one of the roots is equal to the ſum of the other two with their ſigns changed, the other two roots may be repreſented by $-\frac{1}{2}r \pm$ ſome other quantity, ſince the ſum of theſe two, with the ſigns changed, is $= r$. Now this ſupplemental quantity, which

is

is to be connected with $-\frac{1}{2}r$ by the signs + and − to compose the other two roots, will be a real quantity when those roots are real, but an imaginary one when they are imaginary, since the other part $(-\frac{1}{2}r)$ of those two roots is real by the hypothesis. Let this supplemental quantity be represented by e when it is real, or $e\sqrt{-1}$ or $\sqrt{-e^2}$ when it is imaginary: we shall use the quantity e in what follows for the real roots; and it is evident, that by changing e for $e\sqrt{-1}$, or e^2 for $-e^2$, that is, by barely changing the sign of e^2 wherever it is found, the expressions will become adapted to the imaginary roots. Hence then the three roots are represented by r, and $-\frac{1}{2}r+e$, and $-\frac{1}{2}r-e$; and consequently the three equations, from whose continual multiplication by one another the cubic equation is to be generated, will be $x-r=0$, and $x+\frac{1}{2}r-e=0$, and $x+\frac{1}{2}r+e=0$.

9. Let now these three equations be multiplied together, and there will be produced this general cubic equation wanting the second term, namely, $x^3 \begin{smallmatrix}-\frac{3}{4}r^2\\-e^2\end{smallmatrix} x - r\cdot\overline{\frac{1}{4}r^2-e^2}=0$, or $x^3 \begin{smallmatrix}-\frac{3}{4}r^2\\-e^2\end{smallmatrix} x = r\cdot\overline{\frac{1}{4}r^2-e^2}$, having three real roots; and if the sign of e^2 be changed from − to +, it will then represent all the cases which have only one real and two imaginary roots: and from the bare inspection of this equation the following properties are easily drawn.

10. First,

10. First, we hence find, that when the equation has three real roots, the sign of the second term is always $-$; for the coefficient of that term, or p is $= -\frac{3}{4}r^2 - e^2$, which is always negative when r and e are real quantities. And consequently when p is positive, the equation has two imaginary roots, since $-p$ includes all the cases of three real roots. But it does not therefore follow, that when p is negative, the three roots are always real; and indeed there are imaginary roots not only whenever p is positive, but sometimes also when p is negative: for since p is $= -\frac{3}{4}r^2 - e^2$ in all the cases of three real roots, it will be $p = -\frac{3}{4}r^2 + e^2$ for all the cases of two imaginary roots; and it is evident, that p will be either positive or negative, according as e^2 is greater or less than $\frac{3}{4}r^2$.

11. But to find the cases of $-p$ when the roots are all real, and when not, will require some farther consideration; and in order to that it must be observed, that e^2 ought to be positive and less than $\frac{3}{4}r^2$; but the limit between the cases of real and imaginary roots is when $e^2 = 0$, or $e = 0$; and then p becomes $= -\frac{3}{4}r^2$, and $q = \frac{1}{4}r^3$; consequently then $\overline{\frac{1}{3}p}|^3 = \overline{\frac{1}{4}r^2}|^3 = \frac{1}{64}r^6$, which is $= \overline{\frac{1}{2}q}|^2 = \overline{\frac{1}{8}r^3}|^2 = \frac{1}{64}r^6$, that is, when e is $= 0$, then $\overline{\frac{1}{3}p}|^3$ is $= \overline{\frac{1}{2}q}|^2$, and consequently when $\overline{\frac{1}{3}p}|^3$ is less than $\overline{\frac{1}{2}q}|^2$, the equation has two imaginary roots, otherwise none, the sign of p being $-$.

Thus

Thus then we easily perceive in all cases the nature of the roots as to real and imaginary; namely, partly from the sign of p, and partly from the relation of p to q: for the equation has always two imaginary roots when p is positive; it has also two imaginary roots when p is negative, and $\overline{\tfrac{1}{3}p}|^3$ less than $\overline{\tfrac{1}{2}q}|^2$; in the other case the roots are all real, namely, when p is negative and $\overline{\tfrac{1}{3}p}|^3$ either equal to or greater than $\overline{\tfrac{1}{2}q}|^2$.

12. Moreover, when p is $= 0$, the equation has two imaginary roots; for this cannot happen but by $-e^2$ becoming $+e^2$, in the value of p, and $=$ to $\tfrac{3}{4}r^2$; and then $p = -\tfrac{3}{4}r^2 + e^2 = -\tfrac{3}{4}r^2 + \tfrac{3}{4}r^2 = 0$, and $q = r \cdot \overline{\tfrac{1}{4}r^2 + e^2} = r \cdot \overline{\tfrac{1}{4}r^2 + \tfrac{3}{4}r^2} = r \cdot r^2 = r^3$, and consequently the above general equation becomes barely $x^3 = r^3$, which therefore, besides one real root $x = r$, has also two imaginary roots.

13. Hence also it again appears, that the greatest root is always of the same affection, in respect of positive and negative, with q on the right-hand side of the equation, they being either both positive or both negative together; and the other two roots of the contrary sign. For if r be the greatest root, then is $\tfrac{1}{2}r$ greater than e, and $\tfrac{1}{4}r^2$ greater than e^2, and $\tfrac{1}{4}r^2 - e^2$ always positive, and consequently the product $r \cdot \overline{\tfrac{1}{4}r^2 - e^2}$, or q, will have the same sign with r. But if r be one of the less roots, the contrary

trary of this will happen; for then $\frac{1}{2}r$ is leſs than e; and conſequently $\frac{1}{4}r^2$ leſs than e^2, and ſo $\frac{1}{4}r^2 - e^2$ a negative quantity, and therefore the product $r \cdot \overline{\frac{1}{4}r^2 - e^2}$, or q, will have the ſign contrary to that of r; that is, q and the leſs roots have different ſigns, and conſequently q and the greateſt root the ſame ſign, ſince the ſign of the greateſt root is always contrary to that of the other two roots.

14. Moreover, when q or $r \cdot \overline{\frac{1}{4}r^2 - e^2}$ is poſitive, then r denotes the greateſt root; for then $\frac{1}{4}r^2$ is greater than e^2, or $\frac{1}{2}r$ greater than e, and r greater than either $-\frac{1}{2}r + e$ or $-\frac{1}{2}r - e$. But when q or $r \cdot \overline{\frac{1}{4}r^2 - e^2}$ is negative, then r repreſents one of the other two roots in the equation; ſince then e is greater than $\frac{1}{2}r$, and $-\frac{1}{2}r - e$ greater than r. Laſtly, when q is between the poſitive and negative ſtates, or $q = 0$, then r ought to be neither the greateſt nor one of the leſs roots, if I may ſo ſpeak, that is, two of the roots are equal, and the third root $= 0$, ſince then $\frac{1}{4}r^2$ muſt be $= e^2$, or $\frac{1}{2}r = e$.

15. Hence it appears, that the ſign of p determines the nature of the roots as to real and imaginary, and the ſign of q determines the affection of the roots as to poſitive and negative. Let us illuſtrate theſe rules by a few examples.

16. The equation $x^3 - 9x = 10$ has all its three roots real, becauſe $p = -9$ is negative, and $\overline{\frac{1}{3}p}|^3 = 3^3 = 27$ is

 greater

greater than $\overline{\tfrac{1}{2}q}|^2$, which is $= 5^2 = 25$; and the greateſt of the roots is poſitive, becauſe $q = 10$ is poſitive; and the two leſs roots negative.

17. The equation $x^3 - 9x = -10$ has the ſame three real roots as the former, but with the contrary ſigns, the ſign of the greateſt root being now negative, becauſe $q = -10$ is negative.

18. But the equation $x^3 + 9x = \pm 10$ has only one real root and two imaginary roots, becauſe $p = 9$ is poſitive; and the ſign of the real root is + or − according as the ſign of q or 10 is + or −.

19. The equation $x^3 + 6x = \pm 10$ has alſo two imaginary roots, and one real root, which is + or − as it is +10 or −10, for the ſame reaſon as before.

20. The equation $x^3 - 6x = \pm 10$ has alſo two imaginary roots, becauſe $\overline{\tfrac{1}{3}p}|^3 = 2^3 = 8$ is leſs than $\overline{\tfrac{1}{2}q}|^2 = 5^2 = 25$.

21. But the equation $x^3 - 12x = \pm 16$ has all its roots real, becauſe $\overline{\tfrac{1}{3}p}|^3 = 4^3 = 64$ is $= \overline{\tfrac{1}{2}q}|^2 = 8^2 = 64$.

22. And the equation $x^3 + 12x = \pm 16$ has only one real root, becauſe $p = +12$ is poſitive.

23. Let us now conſider the other properties and relations of the roots ariſing from certain aſſumed relations between e and r, and from conſidering e either as real, imaginary, or nothing, that is e^2 as poſitive, negative, or nothing.

24. When

24. When e is a real quantity, the general equation is $x^3 \begin{smallmatrix}-\frac{3}{4}r^2\\ -e^2\end{smallmatrix} x = r \cdot \overline{\tfrac{1}{4}r^2 - e^2}$, and all the roots are real.

25. When e is imaginary, the general equation is $x^3 \begin{smallmatrix}-\frac{3}{4}r^2\\ +e^2\end{smallmatrix} x = r \cdot \overline{\tfrac{1}{4}r^2 + e^2}$, and two of the roots are imaginary.

26. When e is between theſe two ſtates, or $= 0$, the equation becomes $x^3 - \frac{3}{4}r^2 x = \frac{1}{4}r^3$, and the root $r = \sqrt{\frac{4}{3}p} = \sqrt[3]{4q} = \frac{3q}{p}$; for in this caſe $p = \frac{3}{4}r^2$, and $q = \frac{1}{4}r^3$. Alſo the other two roots $-\frac{1}{2}r \pm e$ are each $= -\frac{1}{2}r$.

27. Aſſume now any general relation between the root r and the ſupplemental part e of the other two roots, as ſuppoſe $r^2 : e^2 :: 4 : n$, or $e^2 = \frac{n}{4}r^2$, or $e = \frac{1}{2}r\sqrt{n}$, where n repreſents either nothing or any quantity whether poſitive or negative, that is, poſitive when e and all the three roots are real, or negative when e and two of the roots are imaginary. Subſtitute now $\frac{n}{4}r^2$ inſtead of e^2 in the general equation $x^3 \begin{smallmatrix}-\frac{3}{4}r^2\\ -e^2\end{smallmatrix} x = r \cdot \overline{\tfrac{1}{4}r^2 - e^2}$, and that equation will become $x^3 - \frac{3+n}{4}r^2 x = \frac{1-n}{4}r^3$. Here then $p = \frac{3+n}{4}r^2$, and $q = \frac{1-n}{4}r^3$, and conſequently the root $r = \sqrt{\frac{4p}{3+n}} = \sqrt[3]{\frac{4q}{1-n}} = \frac{n+3}{n-1} \cdot \frac{q}{p}$ expreſſed in three different ways. The other roots, the general values of which are $-\frac{1}{2}r \pm e$, become $-\frac{1}{2}r \pm \sqrt{\frac{n}{4}r^2} = -\frac{1}{2}r \pm \frac{1}{2}r\sqrt{n} = -\frac{1}{2}r \times \overline{1 \pm \sqrt{n}}$.

 28. Hence

28. Hence then in an eaſy and general manner we can repreſent any form or caſe of the general equation, with all the circumſtances of the roots, by only taking, in theſe laſt formulæ, any particular number for n, either poſitive or negative, integral or fractional, &c. As if $n = 1$; then the equation becomes $x^3 - r^2 x = \frac{0}{2} r^3$, or $= 0$, the value of $e = \frac{1}{2} r$, the root $r = \sqrt{p} = \sqrt[3]{\frac{4q}{0}} = \frac{4q}{0p}$, and the other two roots $= -\frac{1}{2} r . \overline{1 \pm \sqrt{1}} = -\frac{1}{2} r . 2$ and $-\frac{1}{2} r . 0 = -r$ and 0.

29. If $n = -1$, the equation will be $x^3 - \frac{1}{2} r^2 x = \frac{1}{2} r^3$, the value of $e = \frac{1}{2} r \sqrt{-1}$, the root $r = \frac{q}{p} = \sqrt{2p} = \sqrt[3]{2q}$, and the other two roots $= -\frac{1}{2} r . \overline{1 \pm \sqrt{1}}$ imaginary.

30. And thus, by taking ſeveral different values of n, poſitive and negative, the various correſponding circumſtances and relations of the equation and roots will be exhibited as in the following table.

Forms

Forms or cases.	Values of n.	Values of e.	Forms of the equation.	Values of the root r, viz. $r =$	Values of the two other roots, viz.
	$+n$	$\frac{1}{2}r\sqrt{+n}$	$x^3 - \frac{n+3}{4}r^2x = -\frac{n-1}{4}r^3$	$\frac{n+3 \cdot q}{n-1 \cdot p} = \sqrt{\frac{4p}{n+3}} = \sqrt[3]{\frac{4q}{n-1}}$	$-\frac{1}{2}r \times$ $1 \pm \sqrt{+n}$
1	$+12$	$\frac{1}{2}r\sqrt{+12}$	$x^3 - \frac{15}{4}r^2x = -\frac{11}{4}r^3$	$\frac{15q}{11p} = \sqrt{\frac{4p}{15}} = \sqrt[3]{\frac{4q}{11}}$	$1 \pm \sqrt{+12}$
2	$+11$	$\frac{1}{2}r\sqrt{+11}$	$x^3 - \frac{14}{4}r^2x = -\frac{10}{4}r^3$	$\frac{14q}{10p} = \sqrt{\frac{4p}{14}} = \sqrt[3]{\frac{4q}{10}}$	$1 \pm \sqrt{+11}$
3	$+10$	$\frac{1}{2}r\sqrt{+10}$	$x^3 - \frac{13}{4}r^2x = -\frac{9}{4}r^3$	$\frac{13q}{9p} = \sqrt{\frac{4p}{13}} = \sqrt[3]{\frac{4q}{9}}$	$1 \pm \sqrt{+10}$
4	$+9$	$\frac{1}{2}r\sqrt{+9}$	$x^3 - \frac{12}{4}r^2x = -\frac{8}{4}r^3$	$\frac{12q}{8p} = \sqrt{\frac{4p}{12}} = \sqrt[3]{\frac{4q}{8}}$	$1 \pm \sqrt{+9}$
5	$+8$	$\frac{1}{2}r\sqrt{+8}$	$x^3 - \frac{11}{4}r^2x = -\frac{7}{4}r^3$	$\frac{11q}{7p} = \sqrt{\frac{4p}{11}} = \sqrt[3]{\frac{4q}{7}}$	$1 \pm \sqrt{+8}$
6	$+7$	$\frac{1}{2}r\sqrt{+7}$	$x^3 - \frac{10}{4}r^2x = -\frac{6}{4}r^3$	$\frac{10q}{6p} = \sqrt{\frac{4p}{10}} = \sqrt[3]{\frac{4q}{6}}$	$1 \pm \sqrt{+7}$
7	$+6$	$\frac{1}{2}r\sqrt{+6}$	$x^3 - \frac{9}{4}r^2x = -\frac{5}{4}r^3$	$\frac{9q}{5p} = \sqrt{\frac{4p}{9}} = \sqrt[3]{\frac{4q}{5}}$	$1 \pm \sqrt{+6}$
8	$+5$	$\frac{1}{2}r\sqrt{+5}$	$x^3 - \frac{8}{4}r^2x = -\frac{4}{4}r^3$	$\frac{8q}{4p} = \sqrt{\frac{4p}{8}} = \sqrt[3]{\frac{4q}{4}}$	$1 \pm \sqrt{+5}$
9	$+4$	$\frac{1}{2}r\sqrt{+4}$	$x^3 - \frac{7}{4}r^2x = -\frac{3}{4}r^3$	$\frac{7q}{3p} = \sqrt{\frac{4p}{7}} = \sqrt[3]{\frac{4q}{3}}$	$1 \pm \sqrt{+4}$
10	$+3$	$\frac{1}{2}r\sqrt{+3}$	$x^3 - \frac{6}{4}r^2x = -\frac{2}{4}r^3$	$\frac{6q}{2p} = \sqrt{\frac{4p}{6}} = \sqrt[3]{\frac{4q}{2}}$	$1 \pm \sqrt{+3}$
11	$+2$	$\frac{1}{2}r\sqrt{+2}$	$x^3 - \frac{5}{4}r^2x = -\frac{1}{4}r^3$	$\frac{5q}{1p} = \sqrt{\frac{4p}{5}} = \sqrt[3]{\frac{4q}{1}}$	$1 \pm \sqrt{+2}$
12	$+1$	$\frac{1}{2}r\sqrt{+1}$	$x^3 - \frac{4}{4}r^2x = \mp \frac{0}{4}r^3$	$\frac{4q}{0p} = \sqrt{\frac{4p}{4}} = \sqrt[3]{\frac{4q}{0}}$	$1 \pm \sqrt{+1}$
13	± 0	$\frac{1}{2}r\sqrt{\pm 0}$	$x^3 - \frac{3}{4}r^2x = +\frac{1}{4}r^3$	$\frac{3q}{1p} = \sqrt{\frac{4p}{3}} = \sqrt[3]{\frac{4q}{1}}$	$1 \pm \sqrt{\pm 0}$
14	-1	$\frac{1}{2}r\sqrt{-1}$	$x^3 - \frac{2}{4}r^2x = +\frac{2}{4}r^3$	$\frac{2q}{2p} = \sqrt{\frac{4p}{2}} = \sqrt[3]{\frac{4q}{2}}$	$1 \pm \sqrt{-1}$
15	-2	$\frac{1}{2}r\sqrt{-2}$	$x^3 - \frac{1}{4}r^2x = +\frac{3}{4}r^3$	$\frac{1q}{3p} = \sqrt{\frac{4p}{1}} = \sqrt[3]{\frac{4q}{3}}$	$1 \pm \sqrt{-2}$
16	-3	$\frac{1}{2}r\sqrt{-3}$	$x^3 \mp \frac{0}{4}r^2x = +\frac{4}{4}r^3$	$\frac{0q}{4p} = \sqrt{\frac{4p}{0}} = \sqrt[3]{\frac{4q}{4}}$	$1 \pm \sqrt{-3}$
17	-4	$\frac{1}{2}r\sqrt{-4}$	$x^3 + \frac{1}{4}r^2x = +\frac{5}{4}r^3$	$\frac{1q}{5p} = \sqrt{\frac{4p}{1}} = \sqrt[3]{\frac{4q}{5}}$	$1 \pm \sqrt{-4}$
18	-5	$\frac{1}{2}r\sqrt{-5}$	$x^3 + \frac{2}{4}r^2x = +\frac{6}{4}r^3$	$\frac{2q}{6p} = \sqrt{\frac{4p}{2}} = \sqrt[3]{\frac{4q}{6}}$	$1 \pm \sqrt{-5}$
19	-6	$\frac{1}{2}r\sqrt{-6}$	$x^3 + \frac{3}{4}r^2x = +\frac{7}{4}r^3$	$\frac{3q}{7p} = \sqrt{\frac{4p}{3}} = \sqrt[3]{\frac{4q}{7}}$	$1 \pm \sqrt{-6}$
20	-7	$\frac{1}{2}r\sqrt{-7}$	$x^3 + \frac{4}{4}r^2x = +\frac{8}{4}r^3$	$\frac{4q}{8p} = \sqrt{\frac{4p}{4}} = \sqrt[3]{\frac{4q}{8}}$	$1 \pm \sqrt{-7}$
21	-8	$\frac{1}{2}r\sqrt{-8}$	$x^3 + \frac{5}{4}r^2x = +\frac{9}{4}r^3$	$\frac{5q}{9p} = \sqrt{\frac{4p}{5}} = \sqrt[3]{\frac{4q}{9}}$	$1 \pm \sqrt{-8}$
22	-9	$\frac{1}{2}r\sqrt{-9}$	$x^3 + \frac{6}{4}r^2x = +\frac{10}{4}r^3$	$\frac{6q}{10p} = \sqrt{\frac{4p}{6}} = \sqrt[3]{\frac{4q}{10}}$	$1 \pm \sqrt{-9}$
23	-10	$\frac{1}{2}r\sqrt{-10}$	$x^3 + \frac{7}{4}r^2x = +\frac{11}{4}r^3$	$\frac{7q}{11p} = \sqrt{\frac{4p}{7}} = \sqrt[3]{\frac{4q}{11}}$	$1 \pm \sqrt{-10}$
24	-11	$\frac{1}{2}r\sqrt{-11}$	$x^3 + \frac{8}{4}r^2x = +\frac{12}{4}r^3$	$\frac{8q}{12p} = \sqrt{\frac{4p}{8}} = \sqrt[3]{\frac{4q}{12}}$	$1 \pm \sqrt{-11}$
25	-12	$\frac{1}{2}r\sqrt{-12}$	$x^3 + \frac{9}{4}r^2x = +\frac{13}{4}r^3$	$\frac{9q}{13p} = \sqrt{\frac{4p}{9}} = \sqrt[3]{\frac{4q}{13}}$	$1 \pm \sqrt{-12}$
	$-n$	$\frac{1}{2}r\sqrt{-n}$	$x^3 + \frac{n-3}{4}r^2x = +\frac{n+1}{4}r^3$	$\frac{n-3 \cdot q}{n+1 \cdot p} = \sqrt{\frac{4p}{n-3}} = \sqrt[3]{\frac{4q}{n+1}}$	$1 \pm \sqrt{-n}$

31. From

31. From the bare inspection of this table several useful and curious observations may be made. And first it appears, that when q is positive, as in all the forms after the 12th, r is the greatest root; but when q is negative, or in all the cases to the 12th, r is one of the less roots.

32. In all cases before the 4th form r is the least root, because $\frac{\sqrt{10}-1}{2}$, or $\frac{\sqrt{11}-1}{2}$, &c. is always greater than 1; and in all such forms $\overline{\tfrac{1}{2}q}|^2$ is less than $\overline{\tfrac{1}{3}p}|^3$; but the former approaches nearer and nearer to an equality with the latter till the 4th form, where $\overline{\tfrac{1}{2}q}|^2$ is become $= \overline{\tfrac{1}{3}p}|^3$, and r is then equal to one of the other roots, because $\frac{\sqrt{9}-1}{2} = \frac{2}{2} = 1$.

33. From hence r becomes the middle root, and continues so to the 12th form, where it becomes equal to what has hitherto been the greatest root, and the other root becomes at this place $= 0$; and $\overline{\tfrac{1}{2}q}|^2$ has decreased from the 4th form all the way more and more in respect of $\overline{\tfrac{1}{3}p}|^3$, till at this 12th form it has become $= 0$, or infinitely less than $\overline{\tfrac{1}{3}p}|^3$.

34. From this place r becomes the greatest root, the sign of q changes to $+$, and $\overline{\tfrac{1}{2}q}|^3$ again increases in respect of $\overline{\tfrac{1}{3}p}|^3$, till at the 13th case it becomes again equal to it, and the two less roots equal to each other, like as at the 4th form.

3 35. From

35. From hence $\overline{\tfrac{1}{2}q}|^2$ becomes greater than $\overline{\tfrac{1}{3}p}|^3$, and increaſes more and more in reſpect of it, till at the 16th ſtep where p is $= 0$, or $\overline{\tfrac{1}{2}q}|^2$ infinitely greater than $\overline{\tfrac{1}{3}p}|^3$.

36. From this place the ſign of p becomes $+$, and $\overline{\tfrac{1}{2}q}|^2$ continually decreaſes in reſpect of $\overline{\tfrac{1}{3}p}|^3$ to infinity.

37. By help of this table we may find the roots of any cubic equation $x^3 \mp px = q$ whenever we can aſſign the relation between $\sqrt{p}$ and $\sqrt[3]{q}$. For ſince one root r is always $= \frac{\overline{n \pm 3} \cdot q}{\overline{n \mp 1} \cdot p} = \sqrt{\frac{4p}{n \pm 3}} = \sqrt[3]{\frac{4q}{n \mp 1}}$, and the other two roots $= -\tfrac{1}{2}r \,.\, \overline{1 \pm \sqrt{\pm n}}$, it follows, that if from the equation $\sqrt{\frac{4p}{n \pm 3}} = \sqrt[3]{\frac{4q}{n \mp 1}}$, where the two denominators under the radicals differ by 4, we can aſſign the value of n, the above formula will give us the roots.

38. As if the equation be $x^3 - 18x = -27$. Here $p = 18$, and $q = 27$; then $\sqrt{\frac{4p}{8}} = \sqrt{\frac{p}{2}} = \sqrt{9} = 3$, and $\sqrt[3]{\frac{4q}{4}} = \sqrt[3]{27} = 3$ alſo; therefore $n + 3 = 8$, or $n - 1 = 4$, either of which gives $n = 5$: conſequently, $r = \frac{\overline{n + 3} \cdot q}{\overline{n - 1} \cdot p}$ $= \frac{8q}{4p} = \frac{2q}{p} = \frac{54}{18} = 3$ is the middle root, becauſe $\frac{8q}{4p}$ is found between the 4th and 12th caſes, which are the limits of the middle roots: and $-\tfrac{1}{2}r \,.\, \overline{1 \pm \sqrt{n}} = -\tfrac{3}{2} \,.\, \overline{1 \pm \sqrt{5}} =$ 4·854102 and 1·854102 are the greateſt and leaſt roots. Or, theſe two roots may be alſo found in the ſame

manner

manner from the table of forms, which contains all the roots of every equation, thus: by a few trials I find $\sqrt{\frac{4p}{20\cdot95}} = \sqrt[3]{\frac{4q}{10\cdot95}}$ nearly, and therefore $\frac{20\cdot95q}{16\cdot95p} = 1\cdot854$ is the leaſt root, becauſe here $n = 17\cdot95$ which lies far above the limit for the leaſt roots, which is at the fourth form, where n is $= 9$. And laſtly, $\sqrt{\frac{4p}{3\cdot0557}} = \sqrt[3]{\frac{4q}{\cdot9143}}$ nearly, and therefore, $\frac{3\cdot0557q}{\cdot9143p} = 4\cdot854$ is the greateſt root, becauſe $\frac{3\cdot0557q}{\cdot9143p}$ is found between the 12th and 13th forms, which are the limits between which lies the greateſt root of every equation that has all its roots real.

39. Again, let the equation be $x^3 + 2x = 12$. Here $p = 2$, and $q = 12$; hence $\sqrt{\frac{4p}{2}} = \sqrt{2p} = \sqrt{4} = 2$, and $\sqrt[3]{\frac{4q}{6}} = \sqrt[3]{\frac{2}{3}q} = \sqrt[3]{8} = 2$ alſo; therefore $n - 3 = 2$, or $n + 1 = 6$, either of which gives $n = 5$. Conſequently, $r = \frac{\overline{n-3}\cdot q}{\overline{n+1}\cdot p} = \frac{2q}{6p} = \frac{q}{3p} = \frac{12}{6} = 2$, and the other two roots are $-\frac{1}{2}r\cdot\overline{1 \pm \sqrt{-n}} = -1\cdot\overline{1 \pm \sqrt{-5}} = -1 \mp \sqrt{-5}$.

40. But it is only by trials that we find out a proper value for n in ſuch caſes as theſe; and this is perhaps attended with no leſs trouble than the ſearching out one of the roots by trials from the original cubic equation itſelf. This method of finding the roots would indeed be effectual and ſatisfactory if we had a direct method of deter-

determining the value of n from the equation $\sqrt{\frac{4p}{n \pm 3}} = \sqrt[3]{\frac{4q}{n \mp 1}}$ by an equation under the 3d degree; but by reducing this equation out of radicals, there reſults another cubic equation of no leſs difficulty to reſolve than the original one. We muſt therefore ſearch for other methods of determining the roots; and firſt it will be proper to treat of the rule which is called CARDAN's.

41. Let $x^3 + px = q$ be the general equation where p and q denote any given numbers with their ſigns, poſitive or negative. And let $z + y$ denote one of the roots of this equation, that is, let the root be divided into any two parts z and y. Hence then $x = z + y$; which value of x being ſubſtituted for it in the original equation $x^3 + px = q$, that equation will become $z^3 + 3z^2y + 3zy^2 + y^3 + p . \overline{z + y} = q$, or $z^3 + y^3 + 3zy . \overline{z + y} + p . \overline{z + y} = q$. Now on introducing the two unknown quantities z and y, we ſuppoſed only one condition or equation, namely, $z + y = x$; we are therefore yet at liberty to aſſume any other poſſible condition we pleaſe: but this other condition ought to be ſuch as will make the equation reducible to a ſimple one, or to a quadratic, in order to obtain from it the value of z or y; and for this purpoſe there does not ſeem to be any other proper condition beſide

that

that which ſuppoſes $3zy$ to be $= -p$; and in conſequence of this ſuppoſition, the equation becomes barely $z^3 + y^3 = q$. Now from the ſquare of this equation let four times the cube of $zy = -\frac{1}{3}p$ be ſubtracted, and there will remain $z^6 - 2z^3y^3 + y^6 = q^2 + \frac{4}{27}p^3$, the ſquare root of which is $z^3 - y^3 = \sqrt{q^2 + \frac{4}{27}p^3}$; this laſt being added to, and ſubtracted from, the equation $z^3 + y^3 = q$, we have

$$\begin{cases} 2z^2 = q + \sqrt{q^2 + \frac{4}{27}p^3} = q + 2\sqrt{\overline{\tfrac{1}{2}q}|^2 + \overline{\tfrac{1}{3}p}|^3}, \\ 2y^3 = q - \sqrt{q^2 + \frac{4}{27}p^3} = q - 2\sqrt{\overline{\tfrac{1}{2}q}|^2 + \overline{\tfrac{1}{3}p}|^3}, \end{cases}$$

hence dividing by 2, and extracting the cube roots, we have

$$\begin{cases} z = \sqrt[3]{\tfrac{1}{2}q + \sqrt{\overline{\tfrac{1}{2}q}|^2 + \overline{\tfrac{1}{3}p}|^3}} \times 1 \text{ or } \times -\frac{1 \pm \sqrt{-3}}{2} \\ y = \sqrt[3]{\tfrac{1}{2}q - \sqrt{\overline{\tfrac{1}{2}q}|^2 + \overline{\tfrac{1}{3}p}|^3}} \times 1 \text{ or } \times -\frac{1 \mp \sqrt{-3}}{2} \end{cases}$$

the three values of z and y; for every quantity has three different forms of the cube root, and the cube root of 1, is not only 1, but alſo $-\frac{1+\sqrt{-3}}{2}$ or $-\frac{1-\sqrt{-3}}{2}$. Hence then the three values of $z + y$ or x, or the three roots of the equation $x^3 + px = q$, are $\sqrt[3]{\tfrac{1}{2}q + \sqrt{\overline{\tfrac{1}{2}q}|^2 + \overline{\tfrac{1}{3}p}|^3}} \times 1$ or $\times -\frac{1+\sqrt{-3}}{2}$ or $\times -\frac{1-\sqrt{-3}}{2} + \sqrt[3]{\tfrac{1}{2}q - \sqrt{\overline{\tfrac{1}{2}q}|^2 + \overline{\tfrac{1}{3}p}|^3}} \times 1$ or $\times -\frac{1-\sqrt{-3}}{2}$ or $\times -\frac{1+\sqrt{-3}}{2}$, where the ſigns of $\sqrt{-3}$ muſt be oppoſite in the values of z and y, that is, when it is $\frac{1 \pm \sqrt{-3}}{2}$ in the one, it muſt be $\frac{1 \mp \sqrt{-3}}{2}$ in the other,

otherwiſe

otherwise their product xy will not be $= -\frac{1}{3}p$, as it ought to be.

42. Or if we put $a = \frac{1}{3}p$, and $b = \frac{1}{2}q$, the same three roots will be

$$\sqrt[3]{b + \sqrt{b^2 + a^3}} + \sqrt[3]{b - \sqrt{b^2 + a^3}} = \text{the 1st root or } r,$$

$$-\tfrac{1}{2}\sqrt[3]{b + \sqrt{b^2 + a^3}} \cdot \overline{1 - \sqrt{-3}} - \tfrac{1}{2}\sqrt[3]{b - \sqrt{b^2 + a^3}} \cdot \overline{1 + \sqrt{-3}} \text{ the 2d root.}$$

$$-\tfrac{1}{2}\sqrt[3]{b + \sqrt{b^2 + a^3}} \cdot \overline{1 + \sqrt{-3}} - \tfrac{1}{2}\sqrt[3]{b - \sqrt{b^2 + a^3}} \cdot \overline{1 - \sqrt{-3}} \text{ the 3d root.}$$

43. Or again, the 1st root r being

$\sqrt[3]{b + \sqrt{b^2 + a^3}} + \sqrt[3]{b - \sqrt{b^2 + a^3}}$, the other two will be

$$-\tfrac{1}{2}r + \frac{\sqrt{-3}}{2}\sqrt[3]{b + \sqrt{b^2 + a^3}} - \frac{\sqrt{-3}}{2}\sqrt[3]{b - \sqrt{b^2 + a^3}} = \text{the 2d root, and}$$

$$-\tfrac{1}{2}r - \frac{\sqrt{-3}}{2}\sqrt[3]{b + \sqrt{b^2 + a^3}} + \frac{\sqrt{-3}}{2}\sqrt[3]{b - \sqrt{b^2 + a^3}} = \text{the 3d root.}$$

44. Or, if we put $s = \sqrt[3]{b + \sqrt{b^2 + a^3}}$, and $d = \sqrt[3]{b - \sqrt{b^2 + a^3}}$, the roots will be

$$s + d = r \text{ the 1st root,}$$

$$-\frac{s+d}{2} + \frac{s-d}{2}\sqrt{-3} = \text{the 2d root,}$$

$$-\frac{s+d}{2} - \frac{s-d}{2}\sqrt{-3} = \text{the 3d root.}$$

45. The first of these roots x or $r = s + d = \sqrt[3]{b + \sqrt{b^2 + a^3}} + \sqrt[3]{b - \sqrt{b^2 + a^3}}$, is that which is called CARDAN's rule, by whom it was first published, but invented by FERREUS. And this is always a real root, though it is not

 always

always the greateſt root as it has been commonly thought to be.

46. The firſt root $r = s + d = \sqrt[3]{b + \sqrt{b^2 + a^3}} + \sqrt[3]{b - \sqrt{b^2 + a^3}}$, although it be always a real quantity, yet often aſſumes an imaginary form when particular numbers are ſubſtituted inſtead of the letters a and b, or p and q. And this it is evident will happen whenever a is negative and a^3 greater than b^2, or $\overline{\tfrac{1}{3}p}|^3$ greater than $\overline{\tfrac{1}{2}q}|^2$; for then $\sqrt{b^2 + a^3}$ becomes $\sqrt{b^2 - a^3} = \sqrt{\overline{\tfrac{1}{2}q}|^2 - \overline{\tfrac{1}{3}p}|^3}$ the ſquare root of a negative quantity, which is imaginary. And this will evidently happen whenever the equation has three real roots, but at no time elſe, that is in all the firſt 13 caſes of the foregoing table, wherein $\overline{\tfrac{1}{3}p}|^3$ is greater than $\overline{\tfrac{1}{2}q}|^2$, and p negative; the 4th and 13th only excepted, when $\overline{\tfrac{1}{3}p}|^3$ is $= \overline{\tfrac{1}{2}q}|^2$, and therefore $\sqrt{b^2 - a^3} = 0$, and two of the roots become equal, but with contrary ſigns. This root can never aſſume an imaginary form when a or p is poſitive, nor yet when p is negative and $\overline{\tfrac{1}{2}q}|^2$ greater than $\overline{\tfrac{1}{3}p}|^3$; for in both theſe caſes the quantity $\sqrt{b^2 \pm a^3}$ is real, or the ſquare root of a poſitive quantity. And theſe take place after the firſt 13 caſes of the table of forms, that is, in all the caſes which have only one real root. So that this rule of CARDAN's always gives the

root

root in an imaginary form when the equation has no imaginary roots, but in the form of a real quantity when it has imaginary roots.

47. It may, perhaps, ſeem wonderful that CARDAN's theorem ſhould thus exhibit the root of an equation under the form of an imaginary or impoſſible quantity always when the equation has no imaginary roots, but at no time elſe; and it may juſtly be demanded what can be the reaſon of ſo curious an accident. But this ſeeming paradox will be cleared up by the following conſideration. It is plain, that this circumſtance muſt have happened either through ſome impropriety in the manner of deducing the values of z and y from the two aſſumed equations $x = z + y$, and $zy = -\frac{1}{3}p$, or elſe by ſome impoſſibility in one of theſe two conditions themſelves; but, on examination, the deductions are found to be all fairly drawn, and the operations rightly performed. The true cauſe muſt therefore lie concealed in one of theſe two conditions $x = z + y$ and $zy = -\frac{1}{3}p$. In the firſt of them it cannot be, becauſe it only ſuppoſes that a quantity x can be divided into two parts z and y, which is evidently a poſſible ſuppoſition: it can therefore no where exiſt but in the latter, namely, $zy = -\frac{1}{3}p$. Now this ſuppoſition is this, that the product of the two parts z and y, into which the conſtant quantity x is divided, is equal

equal to $\frac{1}{3}p$ with its sign changed. But this may always take place when p is positive; for then $-\frac{1}{3}p$ will be negative, and two numbers, the one positive and the other negative, may always be taken such that their product shall be equal to any negative number whatever, and yet their sum be equal to a given quantity x; and this is done by taking the positive one as much greater than x as the other is negative; for thus it is evident the positive and negative numbers may be increased without end: wherefore there is no impossibility in the supposition when p is positive; and therefore then the formula ought to exhibit only real quantities, that is, in all the cases after the 16th in the table of forms, as we have before found. But the same thing cannot always happen when p is negative, or $-\frac{1}{3}p = zy$ is positive: for that zy may be positive, the signs of the two factors z and y must be alike, either both + or both −, that is, both + when the sign of x is +, or both − when that is −: but it is well known, that the greatest product which can be made of the two parts into which a constant quantity x may be divided, is when the parts are equal to each other, or each equal $\frac{1}{2}x$, and therefore the greatest product is equal to $\overline{\frac{1}{2}x}|^2$ or $\frac{1}{4}x^2$: wherefore if $\frac{1}{4}x^2$ be equal to or greater than $-\frac{1}{3}p$, the condition which supposes that zy is $= -\frac{1}{3}p$, is possible, and the formula ought to express the root by real quantities

tities only, otherwise not; but $\frac{1}{4}x^2$, or $\frac{1}{4}r^2$, which is the same thing, is always less than $-\frac{1}{3}p$ in the first thirteen cases of the table of forms; and therefore in all these cases, which are those in which $\overline{\tfrac{1}{3}p}|^3$ is greater than $\overline{\tfrac{1}{2}q}|^2$, or all those which have three real roots, the formula ought to exhibit the root with imaginary quantities, as we have before found to happen; the 4th and 13th cases only excepted, in which $\overline{\tfrac{1}{3}p}|^3$ is $= \overline{\tfrac{1}{2}q}|^2$, and therefore the quantity $\sqrt{b^2 - a^3}$ vanishes, and two of the roots are equal.

48. Thus then the real cause of this circumstance is made manifest, and it is found to be the necessary consequence of the arbitrary hypothesis which was made, which is found to be possible only in certain cases. So that we cannot expect the formula to exhibit a real quantity in the other cases, since an impossible hypothesis must needs lead to an absurd conclusion.

49. The other two roots $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3}$ in their general state, appear in an imaginary form; but on the substitution of numbers for the letters in any example, they come out real or imaginary quantities in those cases in which they ought to be such. For s being $= g + \sqrt{\mp h}$, and $d = g - \sqrt{\mp h}$ according as the roots are all real or only one is such; and $-\frac{s+d}{2} = -g = -\frac{1}{2}r$ always half the one real root, we shall have $\frac{s-d}{2} = \sqrt{\mp h}$ according to

 the

the ſaid two caſes; and conſequently $\frac{s-d}{2}\sqrt{-3} = \sqrt{\pm 3b}$ a real or an imaginary quantity according as the roots are to be real or imaginary.

50. The firſt root r being found from the formula $\sqrt[3]{b + \sqrt{b^2 + a^3}} + \sqrt[3]{b - \sqrt{b^2 + a^3}}$, or by any other means, the other two roots may be exhibited in ſeveral other forms beſides the foregoing, as may be ſhewn in the following manner.

51. The equation being $x^3 + px = q$, and one root r, by ſubſtitution we have $r^3 + pr = q$, and, by ſubtracting, it is - - - $x^3 - r^3 + p \cdot \overline{x - r} = 0$, and, dividing by $x - r$, it becomes $x^2 + rx + r^2 + p = 0$.

Or this ſame equation may be found by barely dividing $x^3 + px - q = 0$ by $x - r = 0$, for the quotient is $x^2 + rx + r^2 + p = 0$. And the reſolution of this quadratic equation gives $x = -\frac{1}{2}r \pm \sqrt{-p - \frac{3}{4}r^2} = -\frac{1}{2}r \pm \frac{1}{2}\sqrt{-4p - 3r^2}$ the other two roots. And from hence again it appears, that theſe two roots are always imaginary when p in the given equation is poſitive; as alſo when it is negative and leſs than $\frac{3}{4}r^2$; which again include all the caſes of the table of forms after the 13th.

52. Again, ſince $r^3 + pr = q$, therefore $r^2 + p = \frac{q}{r}$, and $r^2 = -p + \frac{q}{r}$, and $-3r^2 = 3p - \frac{3q}{r}$; which being

3 ſubſtituted

ſubſtituted in the above value of the two roots, they become $-\frac{1}{2}r \pm \frac{1}{2}\sqrt{-p-\frac{3q}{r}}$.

53. And again, if $-p$ be expelled from this laſt form by means of its value $r^2 - \frac{q}{r}$, the ſame two roots will be expreſſed by $-\frac{1}{2}r \pm \frac{1}{2}\sqrt{r^2 - \frac{4q}{r}} = -\frac{1}{2}r \times \overline{1 \pm \sqrt{1 - \frac{4q}{r^3}}}$.

54. And farther, if r^3 be expelled from this laſt form by means of its value $q - pr$, the ſame two roots will alſo become $-\frac{1}{2}r \times \overline{1 \pm \sqrt{1 - \frac{4q}{q - pr}}} = -\frac{1}{2}r \times \overline{1 \pm \sqrt{\frac{pr + 3q}{pr - q}}}$.

55. We might have derived the above forms in yet another manner thus. The firſt root being r, let the other two roots be v and w: then we ſhall have theſe two equations, namely, $v + w = -r$, and $vwr = q$, or $vw = \frac{q}{r}$; from the ſquare of the firſt of theſe ſubtract four times the laſt, ſo ſhall $v^2 - 2vw + w^2 = r^2 - \frac{4q}{r}$; the root of this is $v - w = \sqrt{r^2 - \frac{4q}{r}}$, which being added to, and taken from $v + w = -r$, and dividing by 2, we have $\left.\begin{matrix} v \\ w \end{matrix}\right\} = -\frac{1}{2}r \pm \frac{1}{2}\sqrt{r^2 - \frac{4q}{r}} = -\frac{1}{2}r \times \overline{1 \pm \sqrt{1 - \frac{4q}{r^3}}}$, the ſame with one of the formulæ above given; and then by ſubſtitution the others will be deduced.

56. To illuſtrate now the rules $x = s + d$, or $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3}$, by ſome examples; ſuppoſe the given

given equation to be $x^3 - 36x = 91$. Here $p = -36$, $q = 91$, $a = \frac{1}{3}p = -12$, $b = \frac{91}{2}$; then $c = \sqrt{b^2 + a^3} = \sqrt{\frac{8281}{4} - 1728} = \sqrt{\frac{1369}{4}} = \frac{37}{2}$, $s = \sqrt[3]{b + c} = \sqrt[3]{\frac{91}{2} + \frac{37}{2}} = \sqrt[3]{64} = 4$, and $d = \sqrt[3]{b - c} = \sqrt[3]{\frac{91}{2} - \frac{37}{2}} = \sqrt[3]{27} = 3$. Consequently, $r = s + d = 4 + 3 = 7$ the first root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = \frac{-7 \pm \sqrt{-3}}{2}$ the other two roots, which are imaginary.

57. Ex. 2. Let the equation be $x^3 + 30x = 117$. Here $a = \frac{1}{3}p = 10$, $b = \frac{1}{2}q = \frac{117}{2}$; then $c = \sqrt{b^2 + a^3} = \sqrt{\frac{13689}{4} + 1000} = \sqrt{\frac{17689}{4}} = \frac{133}{2}$, $s = \sqrt[3]{b + c} = \sqrt[3]{\frac{117}{2} + \frac{133}{2}} = \sqrt[3]{\frac{250}{2}} = \sqrt[3]{125} = 5$, and $d = \sqrt[3]{b - c} = \sqrt[3]{\frac{117}{2} - \frac{133}{2}} = \sqrt[3]{-\frac{16}{2}} = \sqrt[3]{-8} = -2$. Consequently, $r = s + d = 5 - 2 = 3$ the first root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = \frac{-3 \pm 7\sqrt{-3}}{2}$ the other two roots, which are imaginary.

58. Ex. 3. If the equation be $x^3 + 18x = 6$, we shall have $a = 6$, and $b = 3$; then $c = \sqrt{b^2 + a^3} = \sqrt{9 + 216} = \sqrt{225} = 15$, $s = \sqrt[3]{b + c} = \sqrt[3]{3 + 15} = \sqrt[3]{18}$, and $d = \sqrt[3]{b - c} = \sqrt[3]{3 - 15} = \sqrt[3]{-12} = -\sqrt[3]{12}$. Therefore $r = s + d = \sqrt[3]{18} - \sqrt[3]{12} = \cdot 331313$ the first root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = -\frac{\sqrt[3]{18} - \sqrt[3]{12}}{2} \pm \frac{\sqrt[3]{18} + \sqrt[3]{12}}{2}\sqrt{-3}$ the other two roots.

59. Ex. 4. In the equation $x^3 - 15x = 4$, we have $a = -5$, $b = 2$; hence $c = \sqrt{b^2 + a^3} = \sqrt{4 - 125} =$

$\sqrt{-121}$

$\sqrt{-121} = 11\sqrt{-1}$, $s = \sqrt[3]{b+c} = \sqrt[3]{2+11\sqrt{-1}} = 2+\sqrt{-1}$, and $d = \sqrt[3]{b-c} = \sqrt[3]{2-11\sqrt{-1}} = 2-\sqrt{-1}$. Wherefore $r = s+d = 4$ the first root; and

$$-\frac{s+d}{2} \mp \frac{s-d}{2}\sqrt{-3} = -2 \pm \sqrt{-1}\,.\,\sqrt{-3} = -2 \pm \sqrt{3}$$

the other two roots, which are also real.

60. Ex. 5. The equation $x^3 - 6x = 4$ gives $a = -2$, and $b = 2$; therefore $c = \sqrt{b^2+a^3} = \sqrt{4-8} = \sqrt{-4} = 2\sqrt{-1}$, $s = \sqrt[3]{b+c} = \sqrt[3]{2+2\sqrt{-1}} = -1+\sqrt{-1}$, and $d = \sqrt[3]{b-c} = \sqrt[3]{2-2\sqrt{-1}} = -1-\sqrt{-1}$. And hence $r = s+d = -2$ the first root; and

$$-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = 1 \pm \sqrt{-1}\,.\,\sqrt{-3} = 1 \pm \sqrt{3}$$

which are the two extremes, or the greatest and least roots. So that in this example, CARDAN's rule gives the middle root.

61. Ex. 6. Let the equation be $x^3 - 9x = -10$. Then $a = -3$ and $b = -5$; so that $c = \sqrt{b^2+a^3} = \sqrt{25-27} = \sqrt{-2}$, $s = \sqrt[3]{b+c} = \sqrt[3]{-5+\sqrt{-2}} = 1+\sqrt{-2}$, and $d = \sqrt[3]{b-c} = \sqrt[3]{-5-\sqrt{-2}} = 1-\sqrt{-2}$. Hence $r = s+d = 2$ the middle root; and

$$-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = -1 \pm \sqrt{-2}\,.\,\sqrt{-3} = -1 \pm \sqrt{6}$$

the greatest and least roots.

62. Ex. 7. Take the equation $x^3 - 12x = 9$. Here $a = -4$, and $b = \frac{9}{2}$; therefore $c = \sqrt{b^2+a^3} = \sqrt{\frac{81}{4}-64}$ =

$= \sqrt{-\frac{175}{4}} = \frac{5}{2}\sqrt{-7}$, $s = \sqrt[3]{b+c} = \sqrt[3]{\frac{9}{2} + \frac{5}{2}\sqrt{-7}} = -\frac{3}{2} + \frac{1}{2}\sqrt{-7}$, and $d = \sqrt[3]{b-c} = \sqrt[3]{\frac{9}{2} - \frac{5}{2}\sqrt{-7}} = -\frac{3}{2} - \frac{1}{2}\sqrt{-7}$. Hence $r = s + d = -3$ the middle root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = \frac{3}{2} \pm \frac{1}{2}\sqrt{-7} \cdot \sqrt{-3} = \frac{3 \pm \sqrt{21}}{2}$ the greateſt and leaſt roots.

63. Ex. 8. Again, from the equation $x^3 - 12x = -8\sqrt{2}$, we have $a = -4$, and $b = -4\sqrt{2}$; hence $c = \sqrt{b^2 + a^3} = \sqrt{32 - 64} = \sqrt{-32} = 4\sqrt{-2}$, $s = \sqrt[3]{b+c} = \sqrt[3]{-4\sqrt{2} + 4\sqrt{-2}} = \sqrt{2} + \sqrt{-2}$, and $d = \sqrt{2} - \sqrt{-2}$. So that $r = s + d = 2\sqrt{2}$ the middle root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = -\sqrt{2} \pm \sqrt{-2} \cdot \sqrt{-3} = -\sqrt{2} \pm \sqrt{6} = -\sqrt{2} \cdot \overline{1 \mp \sqrt{3}}$ the greateſt and leaſt roots.

64. Ex. 9. But the equation $x^3 - 15x = 22$ gives $a = -5$, and $b = 11$; and therefore $c = \sqrt{b^2 + a^3} = \sqrt{121 - 125} = \sqrt{-4}$, $s = \sqrt[3]{b+c} = \sqrt[3]{11 + \sqrt{-4}} = -1 - \sqrt{-4}$, and $d = -1 + \sqrt{-4}$. Conſequently $r = s + d = -2$ the leaſt root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = 1 \pm \sqrt{-4} \cdot \sqrt{-3} = 1 \pm \sqrt{12}$ the two greater roots.

65. Ex. 10. Laſtly, in the equation $x^3 - 15x = 20$, we have $a = -5$, and $b = 10$; conſequently $c = \sqrt{b^2 + a^3} = \sqrt{100 - 125} = \sqrt{-25} = 5\sqrt{-1}$, $s = \sqrt[3]{b+c} = \sqrt[3]{10 + 5\sqrt{-1}}$, and $d = \sqrt[3]{10 - 5\sqrt{-1}}$. Therefore

$r =$

$r = s + d = \sqrt[3]{10 + 5\sqrt{-1}} + \sqrt[3]{10 - 5\sqrt{-1}} =$ the first root; and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3} = -\frac{\sqrt[3]{10+5\sqrt{-1}}+\sqrt[3]{10-5\sqrt{-1}}}{2}$ $\pm \frac{\sqrt[3]{10+5\sqrt{-1}}-\sqrt[3]{10-5\sqrt{-1}}}{2}\sqrt{-3} =$ the other two roots.

66. Hence it appears, that CARDAN's rule $s + d$ brings out sometimes the greatest root, sometimes the middle root, and sometimes the least root.

Of the Roots by Infinite Series.

67. Another way of assigning the roots of a cubic equation, may be by infinite series, derived from the foregoing formulæ, namely, $s + d$ and $-\frac{s+d}{2} \pm \frac{s-d}{2}\sqrt{-3}$, or $\sqrt[3]{b+c} + \sqrt[3]{b-c}$ and

$-\frac{1}{2} \times \overline{\sqrt[3]{b+c} + \sqrt[3]{b-c}} \pm \frac{1}{2}\sqrt{-3} \times \overline{\sqrt[3]{b+c} - \sqrt[3]{b-c}}.$

For by expanding $\sqrt[3]{b \pm c}$ in an infinite series, we shall evidently have all the roots expressed in such series.

68. Now $s = \sqrt[3]{b+c} = \sqrt[3]{b} \times : 1 + \frac{c}{3b} - \frac{2c^2}{3 \cdot 6b^2} + \frac{2 \cdot 5c^3}{3 \cdot 6 \cdot 9b^3}$ &c.

and $d = \sqrt[3]{b-c} = \sqrt[3]{b} \times : 1 - \frac{c}{3b} - \frac{2c^2}{3 \cdot 6b^2} - \frac{2 \cdot 5c^3}{3 \cdot 6 \cdot 9b^3}$ &c.

Hence $s + d = 2\sqrt[3]{b} \times : 1 - \frac{2c^2}{3 \cdot 6b^2} - \frac{2 \cdot 5 \cdot 8c^4}{3 \cdot 6 \cdot 9 \cdot 12b^4}$ &c.

for the first root, as it was found by Mr. NICOLE, in the

Memoires

Memoires de l'Acad. 1738. Alſo

$s-d=\frac{2c}{\sqrt[3]{b^2}}\times:\frac{1}{3}+\frac{2.5c^2}{3.6.9b^2}+\frac{2.5.8.11c^4}{3.6.9.12.15b^4}$&c. Therefore,

$$\left.\begin{array}{l}-\frac{s+d}{2}\\ \pm\frac{s-d}{2}\sqrt{-3}\end{array}\right\}=\left\{\begin{array}{l}-\sqrt[3]{b}\times:1-\frac{2c^2}{3.6b^2}-\frac{2.5.8c^4}{3.6.9.12b^4}\ \&c.\\ \pm\frac{c\sqrt{-3}}{\sqrt[3]{b^2}}\times:\frac{1}{3}+\frac{2.5c^2}{3.6.9b^2}+\frac{2.5.8.11c^4}{3.6.9.12.15b^4}\ \&c.\end{array}\right.$$

for the other two roots, which were given by CLAIRAUT, in his *Elemens d'Algebre*.

69. Hence again it appears, that when c^2 is poſitive, theſe two latter roots are imaginary; for then the factor $\frac{c\sqrt{-3}}{\sqrt[3]{b^2}}$ is imaginary. And that thoſe roots are real when this c^2 is negative; for then this factor becomes $\frac{c\sqrt{-1}\times\sqrt{-3}}{\sqrt[3]{b^2}}=\frac{c\sqrt{3}}{\sqrt[3]{b^2}}$, a real quantity. But in this laſt caſe, the ſign of every ſecond term in the two ſeries muſt be changed, namely, the ſigns of the terms containing the odd powers of the negative quantity c^2; for the ſeries contain the letters as adapted to the poſitive ſign only.

70. Theſe ſeries are proper for thoſe caſes only in which c^2 is not greater than b^2; for if c^2 were greater than b^2, they would all diverge, and be of no uſe: and the ſeries proper for the other caſes, namely, in which c^2 is greater than b^2, we ſhall give below.

71. That c^2 be leſs than b^2, or the foregoing ſeries be proper to be uſed, a or $\frac{1}{3}p$ muſt be a negative quantity; for if it be poſitive, then $c^2 = b^2 + a^3$ will be greater than b^2. But for this purpoſe a cannot be *any* negative quantity taken at pleaſure; for if it be ſo taken as that a^3 be greater than $2b^2$, then ſhall $-c^2 = a^3 - b^2$ be greater than b^2. And hence theſe ſeries converge only in ſome of the caſes of three real roots, and in ſome of thoſe that have only one real root, namely, from the 16th form to ſomewhere between the 12th and 13th forms in the general table Art. 30. when b is poſitive, and conſequently it includes ſome caſes both with and without imaginary roots. But that in all the caſes, the firſt ſeries $s + d = 2\sqrt[3]{b} \times : 1 - \frac{2c^2}{3 \cdot 6b^2}$ &c. is the greateſt root, as will ſtill more fully appear by conſulting Art. 83.

72. Now, in the firſt place, when $a = 0$, or $c = b$, which is the limit, or 16th caſe in the table Art. 30, the equation being $x^3 = q = 2b$, then the only real root is $s = \sqrt[3]{b + c} = \sqrt[3]{2b} = \sqrt[3]{q} = \sqrt[3]{b} \times : 1 + \frac{1}{3} - \frac{2}{3 \cdot 6} +$ &c.

Hence alſo, dividing by $\sqrt[3]{b}$, we have

$$\sqrt[3]{2} = 1 + \frac{1}{3} - \frac{2}{3 \cdot 6} + \frac{2 \cdot 5}{3 \cdot 6 \cdot 9} - \frac{2 \cdot 5 \cdot 8}{3 \cdot 6 \cdot 9 \cdot 12} \text{ \&c.}$$

73. But in this caſe alſo the root is

$s + d = 2\sqrt[3]{b} \times : 1 - \frac{2}{3 \cdot 6} - \frac{2 \cdot 5 \cdot 8}{3 \cdot 6 \cdot 9 \cdot 12}$ &c. And

conſequently

consequently this is equal to the former series, or $2 \times : 1 - \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12}$ &c. $= 1 + \frac{1}{3} - \frac{2}{3.6} + \frac{2.5}{3.6.9}$ &c. $= \sqrt[3]{2}$. Hence, by subtracting $1 - \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12}$ &c. from both sides, we have

$$1 - \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12} \text{ \&c.} = \frac{1}{3} + \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.}$$

which multiplied by $2\sqrt[3]{b}$ will also give the root of the same equation. And hence, adding $\frac{2}{3.6} + \frac{2.5.8}{3.6.9.12}$ &c. to both sides of the last equation, we find that

$$1 \text{ is } = \frac{1}{3} + \frac{2}{3.6} + \frac{2.5}{3.6.9} + \frac{2.5.8}{3.6.9.12} + \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.}$$

Or, farther, multiplying by 3, and subtracting 1, we have

$$2 = \frac{2}{6} + \frac{2.5}{6.9} + \frac{2.5.8}{6.9.12} + \frac{2.5.8.11}{6.9.12.15} \text{ \&c.}$$

74. Also from $2 \times : 1 - \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12}$ &c. $= \sqrt[3]{2}$ in the last article, we find $\frac{1}{2}\sqrt[3]{2} =$

$$1 - \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12} \text{ \&c.} = \frac{1}{3} + \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.}$$

75. In this case also, namely, $c = b$, the equation $d = \sqrt[3]{b - c} = \sqrt[3]{b} \times : 1 - \frac{c}{3b} - \frac{2c^2}{3.6b^2}$ &c. becomes

$$0 = \sqrt[3]{b} \times : 1 - \frac{1}{3} - \frac{2}{3.6} - \frac{2.5}{3.6.9} \text{ \&c.}$$

And hence, dividing by $\sqrt[3]{b}$, and adding, we have

$$1 = \frac{1}{3} + \frac{2}{3.6} + \frac{2.5}{3.6.9} + \frac{2.5.8}{3.6.9.12} \text{ \&c.}$$

the same as in the last article but one.

76. And

76. And by taking other values of b and c, or other relations between them, any number of infinite series may be assigned, whose sums will be given by the two equations $\sqrt[3]{b \pm c} = \sqrt[3]{b} \times : 1 \pm \frac{c}{3b} - \frac{2c^2}{3 \cdot 6b^2} \pm \frac{2 \cdot 5c^3}{3 \cdot 6 \cdot 9b^3}$ &c. And if b be very great in respect of c, the two first terms of the series will give the cube root true to many places of figures.

77. Hitherto is concerning one of the limits or extreme cases only, namely, when $c^2 = b^2$, or when the equation is $x^3 = q = 2b$. And it has been observed, that the first general series for the three roots converges in all the cases of the equation $x^3 - px = q$, or $x^3 - 3ax = 2b$, in which a^3 is not greater than $2b^2$. But a^3 may be any real quantity not greater than $2b^2$, and so it may be either less than, equal to, or greater than b^2.

78. When, in this equation, a^3 is less than b^2, then c^2 is positive, and less than b^2, and the first series gives the only real root without any change in the signs of the terms. And to this belongs all cases of the equation that can fall in between the 13th and 16th formulæ in the general table in Art. 30.

79. If a^3 be $= b^2$, then $c = 0$, and the three first series give $2\sqrt[3]{b} = \sqrt[3]{4q}$ for the greatest root, and $-\sqrt[3]{b}$ for each of the less roots. The same as at the 13th form in the general table Art. 30.

 80. When

80. When a^3 is greater than b^2, c^2 will be negative, and then, changing the ſigns of the odd powers of c^2, the three general ſeries will give the three roots of the equation, which will always be all real. In this claſs are two caſes, namely, when c^2 is leſs than b^2, and when they are equal, which is the limit; for when c^2 becomes greater than b^2, the ſeries diverge.

81. Now when a^3 is between b^2 and $2b^2$, then c^2 is negative and leſs than b^2, and the general ſeries give all the three real roots by changing the ſign of every other term.

82. And when $a^3 = 2b^2$, then $-c^2 = b^2$, and the three roots become thus:

$2\sqrt[3]{b} \times : 1 + \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12} + \frac{2.5.8.11.14}{3.6.9.12.15.18}$ &c. the firſt or greateſt root,

and $\left\{\begin{array}{l} -\sqrt[3]{b} \times : 1 + \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12} + \frac{2.5.8.11.14}{3.6.9.12.15.18} \text{ \&c.} \\ \pm\sqrt[3]{b} \times \sqrt{3} \times : \frac{1}{3} - \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.} \end{array}\right\}$

the two leſs roots.

83. The firſt of theſe 3 is the greateſt root, becauſe $\sqrt[3]{b} \times : 1 + \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12}$ &c. is greater than $\sqrt[3]{b} \times \sqrt{3} \times : \frac{1}{3} - \frac{2.5}{3.6.9}$ &c. for $1 + \frac{2}{3.6}$ &c. is greater than 1, and $\sqrt{3} \times : \frac{1}{3} - \frac{2.5}{3.6.9}$ &c. $= \sqrt{\tfrac{1}{3}} \times : 1 - \frac{2.5}{6.9}$ &c.

is

is leſs than 1. So that in general the firſt ſeries gives the greateſt of the three roots.

84. But it is evident, that this caſe agrees with the 10th form in the table Art. 30; in which the middle root r is found to be $\sqrt[3]{\frac{4q}{2}} = \sqrt[3]{2q} = -\sqrt[3]{4b} = -2\sqrt[3]{\frac{1}{2}b}$, and the two other, or greateſt and leaſt roots, are $-\frac{1}{2}r \times \overline{1 \pm \sqrt{3}} = \sqrt[3]{\frac{1}{2}b} \times \overline{1 \pm \sqrt{3}}$.

85. Hence by a compariſon of theſe two different forms of the ſame roots we find

$$\frac{\sqrt{3}+1}{2\sqrt[3]{2}} = 1 + \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12} + \frac{2.5.8.11.14}{3.6.9.12.15.18} \text{ \&c.} = \text{A},$$

and $\frac{\sqrt{3}-1}{2\sqrt[3]{2}} = \frac{1}{3} - \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15} - \text{\&c.} = \text{B}.$

86. And by adding and ſubtracting theſe two, we find

$$\frac{\sqrt{3}}{\sqrt[3]{2}} = 1 + \frac{1}{3} + \frac{2}{3.6} - \frac{2.5}{3.6.9} - \frac{2.5.8}{3.6.9.12} + + - - \text{ \&c. and}$$

$$\frac{1}{\sqrt[3]{2}} = 1 - \frac{1}{3} + \frac{2}{3.6} + \frac{2.5}{3.6.9} - \frac{2.5.8}{3.6.9.12} - + + - - \text{ \&c.} = \text{C}.$$

87. Alſo, becauſe $\frac{\sqrt{3}+1}{2\sqrt[3]{2}} \times \frac{\sqrt{3}-1}{2\sqrt[3]{2}}$ is $= \frac{1}{2\sqrt[3]{4}}$, which is $= \frac{1}{2} \times \overline{\frac{1}{\sqrt[3]{2}}}\Big|^2$; therefore the mean proportional between the two ſeries A and B, is to the ſeries C, as the ſide of a ſquare is to its diagonal.

88. Moreover, to and from the two ſeries A and B, adding and ſubtracting the two ſeries in Art. 74. namely,

namely, $\frac{1}{2}\sqrt[3]{2}$ or $\frac{\sqrt[3]{2}}{2} = 1 - \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12}$ &c. $=$ $\frac{1}{3} + \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15}$ &c. we obtain the 4 following series:

$$\frac{\sqrt{3}+\sqrt[3]{4}+1}{4\sqrt[3]{2}} = 1 - \frac{2.5.8}{3.6.9.12} - \frac{2.5.8.11.14.17.20}{3.6.9.12.15.18.21.24} \text{ \&c.}$$

$$\frac{\sqrt{3}+\sqrt[3]{4}-1}{4\sqrt[3]{2}} = \frac{1}{3} + \frac{2.5.8.11}{3.6.9.12.15} + \frac{2.5.8.11.14.17.20.23}{3.6.9.12.15.18.21.24.27} \text{ \&c.}$$

$$\frac{\sqrt{3}-\sqrt[3]{4}+1}{4\sqrt[3]{2}} = \frac{2}{3.6} + \frac{2.5.8.11.14}{3.6.9.12.15.18} \text{ \&c.}$$

$$\frac{\sqrt{3}-\sqrt[3]{4}-1}{4\sqrt[3]{2}} = -\frac{2.5}{3.6.9} - \frac{2.5.8.11.14.17}{3.6.9.12.15.18.21} \text{ \&c.}$$

89. It also appears, that the series

$$1 - \frac{1}{3} + \frac{2}{3.6} + \frac{2.5}{3.6.9} - \frac{2.5.8}{3.6.9.12} - \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.}$$

is the reciprocal of the series

$$1 + \frac{1}{3} - \frac{2}{3.6} + \frac{2.5}{3.6.9} - \frac{2.5.8}{3.6.9.12} + \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.}$$

where the signs of the former series are found by changing the signs of every other pair of terms in the latter; namely, omitting the first term, change the signs of the 2d and 3d terms, then passing over the 4th and 5th terms, change the signs of the 6th and 7th; and so on. For, by Art 86. the former of these series is equal to $\frac{1}{\sqrt[3]{2}}$; and, by Art. 72. the latter is equal to $\sqrt[3]{2}$.

90. Let

90. Let us now consider the cases in which c^2 is greater than b^2, which include all the cases not comprehended by the former, or in which c^2 is not greater than b^2. And this, it is evident, will happen both when a is positive and when negative; namely when a is any positive quantity whatever, or when it is any negative quantity, and a^3 greater than $2b^2$. And in these two classes, c^2 will be positive or negative, according as a is positive or negative.

91. Now the series in this class will be found the same way as in the last, by only writing here the letter c before the letter b; for then we shall have $s = \sqrt[3]{c+b}$, and $d = \sqrt[3]{-c+b} = -\sqrt[3]{c-b}$.

Then $s = \sqrt[3]{c+b} = \sqrt[3]{c} \times : 1 + \frac{b}{3c} - \frac{2b^2}{3 \cdot 6c^2} + \frac{2 \cdot 5b^3}{3 \cdot 6 \cdot 9c^3}$ &c.

and $d = -\sqrt[3]{c-b} = \sqrt[3]{c} \times : -1 + \frac{b}{3c} + \frac{2b^2}{3 \cdot 6c^2} + \frac{2 \cdot 5b^3}{3 \cdot 6 \cdot 9c^3}$ &c.

Hence $s + d = \frac{2b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2 \cdot 5b^2}{3 \cdot 6 \cdot 9c^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11b^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15c^4}$ &c. = the 1st root, and was given by CLAIRAUT. And

$$\left.\begin{array}{l} -\frac{s+d}{2} \\ \pm \frac{s-d}{2}\sqrt{-3} \end{array}\right\} = \left\{\begin{array}{l} \frac{-b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2 \cdot 5b^2}{3 \cdot 6 \cdot 9c^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11b^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15c} \text{ \&c.} \\ \pm \sqrt[3]{c} \cdot \sqrt{-3} \times : 1 - \frac{2b^2}{3 \cdot 6c^2} - \frac{2 \cdot 5 \cdot 8b^4}{3 \cdot 6 \cdot 9 \cdot 12c^4} \text{ \&c.} \end{array}\right.$$

for the other two roots, which, I believe, are new.

92. Here it again appears, that when c^2 is positive, the two latter roots are imaginary; because then

$\sqrt[3]{c} \times \sqrt{-3}$ will be imaginary. But if c^2 be negative, thoſe roots will be both real; ſince $\sqrt[3]{c} \times \sqrt{-3}$ then becomes $\sqrt[3]{\overline{c \,.\, \sqrt{-1}}} \times \sqrt{-3} = \sqrt[3]{c} \times -\sqrt{-1} \times \sqrt{-3} = -\sqrt[3]{c} \times \sqrt{3}$. The ſigns prefixed to the terms as above, take place when c^2 is poſitive; but when c^2 ſhall be negative, the ſigns of the terms containing the odd powers of it muſt be changed. And theſe ſeries include all the caſes in which the former ones failed by not converging. So that between them they comprehend all the caſes of the general cubic equation $x^3 \pm px = q$, as they each reciprocally converge when the other diverges, but in no other caſe, except in the common claſs, in which c is $= b$, which happens at the two limits, namely, either when a is $= 0$, or when $-a^3 = 2b^2$: and then they both give the ſame roots. But in the other caſes they give the contrary roots; namely, when c is leſs than b, the firſt ſeries gives the greateſt root; and when c is greater than b, the latter ſeries gives the leaſt root.

93. Now when a is any poſitive quantity, the firſt of theſe ſeries gives the only real root, without any change in the ſigns of the terms; the other two being imaginary. And this includes all the caſes after the 16th in the table in Art. 30.

94. When a is $= 0$, or the limit between poſitive and negative, as in the 16th form in Art. 30. then is $c = b$,

and the only real root, or the first series, becomes $2\sqrt[3]{b} \times : \frac{1}{3} + \frac{2.5}{3.6.9} +$ &c. which is the same root as was before found in Art. 73. So that in this 16th case, both this series and the series in Art. 67. converge, and give the same and only real root.

95. When a becomes negative, then c^2 becomes negative, and the roots all real. But in this case the series only begins to converge when $-a^3 = 2b^2$, for then $-c^2$ becomes $= b^2$, and then, making the proper change in the signs of the terms, the three roots become

1st. $-2\sqrt[3]{b} \times : \frac{1}{3} - \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15}$ &c. the least root, and

$$\begin{cases} +\sqrt[3]{b} \times : \frac{1}{3} - \frac{2.5}{3.6.9} + \frac{2.5.8.11}{3.6.9.12.15} \text{ \&c.} \\ \pm \sqrt[3]{b}.\sqrt{3} \times : 1 + \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12} + \frac{2.5.8.11.14}{3.6.9.12.15.18} \text{ \&c.} \end{cases}$$

the two greater roots.

96. I have here said, that the first of these three roots is the least of them. To prove which, I assert, that $\sqrt{3} \times : 1 + \frac{2}{3.6} - \frac{2.5.8}{3.6.9.12}$ &c. is greater than 3 times $\frac{1}{3} - \frac{2.5}{3.6.9}$ &c. for $3 \times : \frac{1}{3} - \frac{2.5}{3.6.9}$ &c. $= 1 - \frac{2.5}{6.9}$ &c. is less than 1, whereas $1 + \frac{2}{3.6}$ &c. is greater than 1. Consequently, the less of the two latter roots, namely,

$\sqrt[3]{b}$.

$\sqrt[3]{b} \cdot \sqrt{3} \times : 1 + \frac{2}{3 \cdot 6}$ &c. $- \sqrt[3]{b} \times : \frac{1}{3} - \frac{2 \cdot 5}{3 \cdot 6 \cdot 9}$ &c. is greater than the first root $2\sqrt[3]{b} \times : \frac{1}{3} - \frac{2 \cdot 5}{3 \cdot 6 \cdot 9}$ &c. That is to say, here the first is the least of the three roots, while in the other class of series the first is the greatest root.

97. Hence, comparing the value of any one of the roots here found, with the value of the same root as found in Art. 82, we obtain the relation between the two series that are concerned in them, namely, that the series $1 + \frac{2}{3 \cdot 6} - \frac{2 \cdot 5 \cdot 8}{3 \cdot 6 \cdot 9 \cdot 12} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 14}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 18}$ &c. is to the series $\frac{1}{3} - \frac{2 \cdot 5}{3 \cdot 6 \cdot 9} + \frac{2 \cdot 5 \cdot 8 \cdot 11}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15} - \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 14 \cdot 17}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 18 \cdot 21}$ &c. as $\sqrt{3} + 1$ is to $\sqrt{3} - 1$, or as $2 + \sqrt{3}$ to 1, or as 1 to $2 - \sqrt{3}$, which are all equal to the same ratio. And the same thing appears from Art. 85.

98. When $-a^3$ becomes greater than $2b^2$, $-c^2$ is greater than b^2, and, by the proper change in the signs, the series for the roots in all cases of this kind become

1st. $\frac{-2b}{\sqrt[3]{c^2}} \times : \frac{1}{3} - \frac{2 \cdot 5b^2}{3 \cdot 6 \cdot 9c^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11b^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15c^4}$ &c. the least root.

$$\text{and} \left\{ \begin{array}{l} + \frac{b}{\sqrt[3]{c^2}} \times : \frac{1}{3} - \frac{2 \cdot 5b^2}{3 \cdot 6 \cdot 9c^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11b^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15c^4} \text{ \&c.} \\ \pm \sqrt[3]{c} \cdot \sqrt{3} \times : 1 + \frac{2b^2}{3 \cdot 6c^2} - \frac{2 \cdot 5 \cdot 8b^4}{3 \cdot 6 \cdot 9 \cdot 12c^4} \text{ \&c.} \end{array} \right\} \text{the two greater roots.}$$

99. Let us now illustrate all the foregoing series for the roots of cubic equations, by finding by means of them

them the roots of the equations already treated of in Art. 56, &c.

100. And first in the equation $x^3 - 36x = 91$. Here $p = -36$, $q = 91$, $a = -12$, $b = 45\frac{1}{2}$, $c^2 = b^2 + a^3$ $= \overline{45\frac{1}{2}}|^2 - 12^3 = 342\frac{1}{4}$, which being positive and less than b^2, this case belongs to the series

$2\sqrt[3]{b} \times : 1 - \frac{2c^2}{3 . 6b^2} - \frac{2 . 5 . 8c^4}{3 . 6 . 9 . 12b^4} -$ &c. in Art. 68.

Now $\frac{c^2}{b^2} = \frac{1369}{8281} = \overline{\frac{37}{91}}|^2 = \cdot 1653182$. Then

$A =$		$+\ 1\cdot 0000000$
$B = \frac{2c^2}{3 . 6b^2} A$	$= -$	$\cdot 0183687$
$C = \frac{5 . 8c^2}{9 . 12b^2} B$	$= -$	11247
$D = \frac{11 . 14c^2}{15 . 18b^2} C$	$= -$	1061
$E = \frac{17 . 20c^2}{21 . 24b^2} D$	$= -$	118
$F = \frac{23 . 26c^2}{27 . 30b^2} E$	$= -$	14
$G = \frac{29 . 32c^2}{33 . 36b^2} F$	$= -$	2
sum of the terms $=$		$\cdot 9803871$
		2
		$1\cdot 9607742$ - log. $0\cdot 2924275$

$\sqrt[3]{b} = \sqrt[3]{45\cdot 5}$ - - - - - $0\cdot 5526705$

hence the only real root is 7 - - - $0\cdot 8450980$

That

That is, $x = 7$ is $= 2\sqrt[3]{\frac{91}{2}} \times : 1 - \frac{2 \cdot 37^2}{3 \cdot 6 \cdot 91^2} - \frac{2 \cdot 5 \cdot 8 \cdot 37^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 91^4}$ — &c.

101. The other two roots are imaginary, and in Art. 56 they were found to be $= \frac{-7 \pm \sqrt{-3}}{2}$; but by means of the series in Art. 68, they are here found to be $\frac{-7}{2} \pm \frac{c\sqrt{-3}}{\sqrt[3]{b^2}} \times : \frac{1}{3} + \frac{2 \cdot 5c^2}{3 \cdot 6 \cdot 9b^2} +$ &c.

Consequently we obtain these following sums:

$$\frac{7}{2}\sqrt[3]{\frac{2}{91}} = 1 - \frac{2 \cdot 37^2}{3 \cdot 6 \cdot 91^2} - \frac{2 \cdot 5 \cdot 8 \cdot 37^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 91^4} - \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 14 \cdot 37^6}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 18 \cdot 91^6} \text{ \&c.}$$

$$\frac{1}{37}\sqrt[3]{\frac{91^2}{2^2}} = \frac{1}{3} + \frac{2 \cdot 5 \cdot 37^2}{3 \cdot 6 \cdot 9 \cdot 91^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 37^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 91^4} \text{ \&c.}$$

102. Ex. 2. In the equation $x^3 + 30x = 117$, we have $a = \frac{1}{3}p = 10$, $b = \frac{1}{2}q = \frac{117}{2} = 58\frac{1}{2}$, and $c^2 = b^2 + a^3 = \overline{\frac{133}{2}}|^2$, which being positive, and greater than b^2, the proper series for this is that in Art. 91, namely,

$$x = \frac{2b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2 \cdot 5b^2}{3 \cdot 6 \cdot 9c^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11b^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15c^4} + \text{\&c.}$$

Now $\frac{b^2}{c^2} = \overline{\frac{117}{133}}|^2 = \cdot 7738308$. Hence

A =

$$A = \frac{1}{3} \qquad = \cdot 333$$
$$B = \frac{2 \cdot 5 b^2}{6 \cdot 9 c^2} A = \quad 48$$
$$C = \frac{8 \cdot 11 b^2}{12 \cdot 15 c^2} B = \quad 18$$
$$D = \frac{14 \cdot 17 b^2}{18 \cdot 21 c^2} C = \quad 9$$
$$E = \frac{20 \cdot 23 b^2}{24 \cdot 27 c^2} D = \quad 5$$
$$F = \frac{26 \cdot 29 b^2}{30 \cdot 33 c^2} E = \quad 3$$
$$G = \frac{32 \cdot 35 b^2}{36 \cdot 39 c^2} F = \quad 2$$
$$H = \frac{38 \cdot 41 b^2}{42 \cdot 45 c^2} G = \quad 1$$
$$I = \frac{44 \cdot 47 b^2}{48 \cdot 51 c^2} H = \quad 1$$
$$K = \frac{50 \cdot 53 b^2}{54 \cdot 57 c^2} I = \quad 1$$

sum of the terms $= \cdot 421$

$$\frac{2b}{\sqrt[3]{c^2}} = 7 \cdot 128$$

series inverted $\quad 124$

$$2851$$
$$142$$
$$7$$

the root $x = 3 \cdot 000$

That is, $x = 3 = \frac{2 \cdot 117}{\sqrt[3]{2 \cdot 133^2}} \times : \frac{1}{3} + \frac{2 \cdot 5 \cdot 117^2}{3 \cdot 6 \cdot 9 \cdot 133^2} + \&c.$

103. By the other series in the same article the two imaginary roots come out

$= -\frac{3}{2} \pm \sqrt[3]{c} \cdot \sqrt{-3} \times : 1 - \frac{2b^2}{3 \cdot 6 c^2} - \&c.$ which were before found in Art. 57 to be $-\frac{3}{2} \pm \frac{7}{2}\sqrt{-3}$. Consequently

$$\frac{7}{2}\sqrt[3]{\frac{2}{133}} = 1 - \frac{2 \cdot 117^2}{3 \cdot 6 \cdot 133^2} - \frac{2 \cdot 5 \cdot 8 \cdot 117^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 133^4} - \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 14 \cdot 117^6}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 18 \cdot 133^6} \&c.$$

$$\frac{1}{39}\sqrt[3]{\frac{133^2}{2^2}} = \frac{1}{3} + \frac{2 \cdot 5 \cdot 117^2}{3 \cdot 6 \cdot 9 \cdot 133^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 117^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 133^4} + \&c.$$

104. Ex. 3. In the equation $x^3 + 18x = 6$, we have $a = 6$, $b = 3$, $c = \sqrt{9 + 216} = \sqrt{225} = 15$, real and greater than b, and therefore this cafe belongs to the fame feries as the laft example. Now $\frac{b^2}{c^2} = \frac{9}{225} = \frac{1}{25} = \cdot 04$, and $\frac{2b}{\sqrt[3]{c^2}} = \frac{6}{\sqrt[3]{225}} = \sqrt[3]{\frac{24}{25}} = \frac{1}{5}\sqrt[3]{120} = \sqrt[3]{\cdot 96}$. Then

$$\begin{array}{llr}
A = \frac{1}{3} & = & \cdot 3333333 \\
B = \frac{2 \cdot 5 b^2}{6 \cdot 9 c^2} A & = & 24692 \\
C = \frac{8 \cdot 11 b^2}{12 \cdot 15 c^2} B & = & 483 \\
D = \frac{14 \cdot 17 b^2}{18 \cdot 21 c^2} C & = & 12 \\
\hline
\end{array}$$

$$\begin{array}{lll}
& \cdot 3358520 \quad - \quad \log. & \bar{1}\cdot 5261480 \\
& \sqrt[3]{\cdot 96} \quad - \quad - \quad - \quad - & \bar{1}\cdot 9940904 \\
\hline
\text{the root } x = & \cdot 3313130 \quad - \quad - & \bar{1}\cdot 5202384
\end{array}$$

And then the two imaginary roots are

$$-\frac{\cdot 331313}{2} \pm \sqrt[3]{c} \cdot \sqrt{-3} \times : 1 - \frac{2b^2}{3 \cdot 6c^2} \text{ \&c.}$$

105. But, in Art. 58, thefe three roots were found to be $\sqrt[3]{18} - \sqrt[3]{12}$, and $-\frac{\sqrt[3]{18} - \sqrt[3]{12}}{2} \pm \frac{\sqrt[3]{18} + \sqrt[3]{12}}{2}\sqrt{-3}$. Confequently we have

$$\frac{\sqrt[3]{18} + \sqrt[3]{12}}{2\sqrt[3]{15}} = 1 - \frac{2}{3 \cdot 6 \cdot 25^2} - \frac{2 \cdot 5 \cdot 8}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 25^4} - \text{\&c.}$$

$$\frac{\sqrt[3]{18} - \sqrt[3]{12}}{2} \sqrt[3]{\frac{25}{3}} = \frac{1}{3} + \frac{2 \cdot 5}{3 \cdot 6 \cdot 9 \cdot 25^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 25^4} + \text{\&c.}$$

106. Ex

106. Ex. 4. In the equation $x^3 - 15x = 4$, we have $a = -5$, $b = 2$, and $c = \sqrt{b^2 + a^3} = \sqrt{-121} = 11\sqrt{-1}$, imaginary and greater than b, which belongs to the same series as the last 2 examples, but changing the sign where the odd powers of the negative quantity c^2 is concerned, as in Art. 98.

Now $\frac{b^2}{c^2} = \frac{2^2}{11^2} = \frac{4}{121}$, and $\frac{2b}{\sqrt[3]{c^2}} = \frac{4}{\sqrt[3]{121}} = \sqrt[3]{\frac{64}{121}}$. Then

	+		−
$A = \frac{1}{3}$	$=$ ·3333333	$B = \frac{2 \cdot 5b^2}{6 \cdot 9c^2} A$	$=$ ·0020406
$C = \frac{8 \cdot 11b^2}{12 \cdot 15c^2} B =$	330	$D = \frac{14 \cdot 17b^2}{18 \cdot 21c^2} C =$	7
	+ ·3333663		− ·0020413
	− ·0020413		

the series $=$ ·3313250 - log. $\bar{1}$·5202543

$\sqrt[3]{\frac{64}{121}}$ - - - $\bar{2}$·9077982

the least root $= -$·2679492 - - $\bar{1}$·4280525

107. To find the other roots by this method, we must sum the series $\sqrt[3]{c} \cdot \sqrt{3} \times : 1 + \frac{2b^2}{3 \cdot 6c^2} -$ &c. And as the terms of it are found by multiplying the terms A, B, C, &c. of the former by $\frac{3}{1}$, $\frac{9}{5}$, $\frac{15}{11}$, $\frac{21}{17}$, &c. respectively, we shall therefore have

$\alpha =$

$$\begin{array}{lll|l}
\alpha = \frac{3}{1}A & = 1 & & \\
\beta = \frac{9}{5}B & = 0{\cdot}0036731 & & \gamma = \frac{15}{11}C = -{\cdot}0000450 \\
\delta = \frac{21}{17}D = & 8 & &
\end{array}$$

$$\begin{array}{rl}
+ & 1{\cdot}0036739 \\
- & 0000450
\end{array}$$

$$\begin{array}{llll}
\text{series} = + 1{\cdot}0036289 & - & \text{log.} & 0{\cdot}0015732 \\
& \sqrt[3]{11} & - - - - & 0{\cdot}3471309 \\
& \sqrt{3} & - - - - & 0{\cdot}2385606 \\
& \pm 3{\cdot}8660254 & - & 0{\cdot}5872647 \\
\left.\begin{array}{l}\text{the least root}\\ \text{with a contr. sign}\end{array}\right\} & + 0{\cdot}1339746 & & \\
\text{sum} & + 4{\cdot}0000000 & \text{greatest root} & \\
\text{diff.} & - 3{\cdot}7320508 & \text{middle root.} &
\end{array}$$

108. But the same 3 roots, found in Art. 59, are also 4, and $-2 \pm \sqrt{3}$; which being compared with the series in this example, we find

$$\frac{1+2\sqrt{3}}{2\sqrt[3]{11}} = 1 + \frac{2 \cdot 2^2}{3 \cdot 6 \cdot 11^2} - \frac{2 \cdot 5 \cdot 8 \cdot 2^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 11^4} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 14 \cdot 2^6}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 18 \cdot 11^6} \ \&c.$$

$$\frac{2-\sqrt{3}}{4}\sqrt[3]{121} = \frac{1}{3} - \frac{2 \cdot 5 \cdot 2^2}{3 \cdot 6 \cdot 9 \cdot 11^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 2^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 11^4} - \&c.$$

109. Ex. 5. In the equation $x^3 - 6x = 4$, we have $a = -2$, $b = 2$, and $c^2 = b^2 + a^3 = 4 - 8 = -4$, which being negative, and $= b^2$, this case belongs to the series either in Art. 82 or 95. The operation of summing the terms by them is here omitted, because so much room

room would be neceſſary to ſet down ſo great a number of terms, and as the properties ariſing from the ſeries in this caſe have already been noticed above. The 3 roots of this equation have been found in Art. 60 to be -2 and $1 \pm \sqrt{3}$.

110. Ex. 6. In the equation $x^3 - 9x = -10$, we have $a = -3$, $b = -5$, and $c^2 = 25 - 27 = -2$, which being negative and leſs than b^2, the general ſeries in Art. 68, with the neceſſary change of the ſigns, will give the 3 roots. Now $\frac{c^2}{b^2} = \frac{2}{25} = \frac{8}{100} = \cdot 08$, and $\sqrt[3]{b} = -\sqrt[3]{5}$, alſo $\frac{c\sqrt{-3}}{\sqrt[3]{b^2}} = \frac{\sqrt{6}}{\sqrt[3]{25}}$. Hence

$$\begin{array}{lll|lll}
A = & & = 1 & & & \\
B = \frac{2c^2}{3 \cdot 6b^2} A & = & 0\cdot0088889 & C = \frac{5 \cdot 8c^2}{9 \cdot 12b^2} B & = & \cdot0002634 \\
D = \frac{11 \cdot 14c^2}{15 \cdot 18b^2} C & = & 120 & E = \frac{17 \cdot 20c^2}{21 \cdot 24b^2} D & = & 7 \\
\hline
 & + & 1\cdot0089009 & & - & \cdot0002641 \\
 & - & 0\cdot0002641 & & & \\
\hline
 & + & 1\cdot0086368 & & & \\
 & & 2 & & & \\
\hline
\end{array}$$

$$\begin{array}{lll}
 & 2\cdot0172736 \text{ - - log.} & 0\cdot3047649 \\
 & \sqrt[3]{b} = \sqrt[3]{5} \text{ - - - -} & 0\cdot2329900 \\
\hline
\text{the greateſt root} = -3\cdot44948974 & \text{-} & 0\cdot5377549
\end{array}$$

111. Then

111. Then for the other roots, by multiplying the terms A, B, C, &c. of the former by $\frac{1}{3}$, $\frac{5}{9}$, $\frac{11}{15}$, &c. we have

$\alpha = \frac{1}{3}A =$	$\cdot3333333$	$\beta = \frac{5}{9}B =$	$\cdot0049383$
$\gamma = \frac{11}{15}C =$	1931	$\delta = \frac{17}{21}D =$	97
$\varepsilon = \frac{23}{27}E =$	6		$-\cdot0049480$

$$
\begin{array}{lrlr}
 & +\ \cdot3335270 & & \\
 & -\ \cdot0049480 & & \\
\hline
 & \cdot3285790 & \text{- - - log.} & \bar{1}\cdot5166398 \\
 & \dfrac{\sqrt{6}}{\sqrt[3]{25}} & \text{- - - - - -} & \bar{1}\cdot9230956 \\
\hline
\text{the second series} & \pm\ \cdot27525513 & \text{- -} & \bar{1}\cdot4397354 \\
\frac{1}{2}\text{ the greatest root} & +\ 1\cdot72474487 & & \\
\hline
\text{middle root} & 2\cdot00000000 & & \\
\text{least root} & 1\cdot44948974 & &
\end{array}
$$

112. But, by Art. 61, these 3 roots were found to be 2 and -1 ± 6; which being compared with the series belonging to this case, we find

$$\frac{\sqrt{6}+1}{2\sqrt[3]{5}} = 1 + \frac{2.2}{3.6.25} - \frac{2.5.8.2^2}{3.6.9.12.25^2} + \frac{2.5.8.11.14.2^3}{3.6.9.12.15.18.25^3} \text{ \&c.}$$

$$\frac{\sqrt{6}-2}{4}\sqrt[3]{25} = \frac{1}{3} - \frac{2.5.2}{3.6.9.25} + \frac{2.5.8.11.2^2}{3.6.9.12.15.25^2} - \text{\&c.}$$

113. Ex. 7. In the equation $x^3 - 12x = 9$, we have $a = -4$, $b = \frac{9}{2}$, and $c^2 = \frac{81}{4} - 64 = -\frac{175}{4}$, which being negative, and greater than b^2, we shall have 3 real roots by the series in Art. 98.

Now $\frac{b^2}{c^2} = \frac{81}{175}$, $\frac{b}{\sqrt[3]{c^2}} = \frac{9}{\sqrt[3]{350}} = \sqrt[3]{\frac{729}{350}}$, and

$\sqrt[3]{c} = \sqrt[6]{\frac{175}{4}} = \sqrt[6]{43{\cdot}75}$. Then

$A = \frac{1}{3}$	$= {\cdot}33333$	$B = \frac{2 \cdot 5 b^2}{6 \cdot 9 c^2} A$	$= {\cdot}02857$
$C = \frac{8 \cdot 11 b^2}{12 \cdot 15 c^2} B =$	647	$D = \frac{14 \cdot 17 b^2}{18 \cdot 21 c^2} C =$	188
$E = \frac{20 \cdot 23 b^2}{24 \cdot 27 c^2} D =$	62	$F = \frac{26 \cdot 29 b^2}{30 \cdot 33 c^2} E =$	22
$G = \frac{32 \cdot 35 b^2}{36 \cdot 39 c^2} F =$	8	$H = \frac{38 \cdot 41 b^2}{42 \cdot 45 c^2} G =$	3
$I = \frac{44 \cdot 47 b^2}{48 \cdot 51 c^2} H =$	1	$K = \frac{50 \cdot 53 b^2}{54 \cdot 57 c^2} I =$	1
	$+ {\cdot}34051$		$- {\cdot}03071$

$$\begin{array}{rl} - {\cdot}03071 & \\ \hline {\cdot}30980 & \\ 2 & \\ \hline {\cdot}61960 & \text{- - log. } \bar{1}{\cdot}7921114 \\ \sqrt[3]{\frac{729}{350}} & \text{- - - } 0{\cdot}1062198 \\ \hline \end{array}$$

the leaſt root $= - {\cdot}79128$ - - $\bar{1}{\cdot}8983312$

114. Then, ſince the terms of the latter ſeries are found by multiplying the terms of the former by the fractions $\frac{3}{1}$, $\frac{9}{5}$, $\frac{15}{11}$, $\frac{21}{17}$, &c. they will be thus:

$\alpha = \frac{3}{1}A$ =	1·00000	$\gamma = \frac{15}{11}C$ =	·00882
$\beta = \frac{9}{5}B$ =	5143	$\varepsilon = \frac{27}{23}E$ =	73
$\delta = \frac{21}{17}D$ =	232	$\eta = \frac{39}{35}G$ =	9
$\zeta = \frac{33}{29}F$ =	25	$\iota = \frac{51}{47}I$ =	1
$\theta = \frac{45}{41}H$ =	4		− ·00965
	+ 1·05404		
	− 0·00965		

1·04439	- - - log.	0·0188627
$\sqrt[6]{43\cdot75}$	- - - - - -	0·2734963
$\sqrt{3}$	- - - - - - -	0·2385606
last series ± 3·39564	- -	0·5309196
$-\frac{1}{2}$ the first + 0·39564		
greatest root + 3·79128		
middle root − 3·00000		

115. But, by Art. 62, these same 3 roots are, − 3, and $\frac{3 \pm \sqrt{21}}{2}$; which being compared with the series belonging to this case, we find

$$\frac{\sqrt{21}+9}{12\sqrt[6]{350}}\sqrt{6} = 1 + \frac{2\,.\,81}{3\,.\,6\,.\,175} - \frac{2\,.\,5\,.\,8\,.\,81^2}{3\,.\,6\,.\,9\,.\,12\,.\,175^2} + \text{\&c.}$$

$$\frac{\sqrt{21}-3}{36}\sqrt[3]{350} = \frac{1}{3} - \frac{2\,.\,5\,.\,81}{3\,.\,6\,.\,9\,.\,175} + \frac{2\,.\,5\,.\,8\,.\,11\,.\,81^2}{3\,.\,6\,.\,9\,.\,12\,.\,15\,.\,175^2} - \text{\&c.}$$

116. Ex. 8. In the equation $x^3 - 12x = -8\sqrt{2}$, we have $a = -4$, $b = -4\sqrt{2}$, and $c^2 = 32 - 64 = -32$, which being negative, and equal to b^2, the 3 roots will be

be found, by both the forms of series, like as in Ex. 5, Art. 109; but the operation is here omitted for the same reasons as were there given. The 3 roots of this equation were, in Art. 63, found to be $2\sqrt{2}$ and $-\sqrt{2}\pm\sqrt{6}$.

117. Ex. 9. In the equation $x^3 - 15x = 22$, we have $a = -5$, $b = 11$, and $c^2 = 121 - 125 = -4$, which being negative, and less than b^2, the series in Art. 68 give these 3 roots:

$$\text{Greatest root} = 2\sqrt[3]{11} \times : 1 + \frac{2c^2}{3 \cdot 6b^2} - \frac{2 \cdot 5 \cdot 8c^4}{3 \cdot 6 \cdot 9 \cdot 12b^4} \text{ \&c.}$$

$$\text{The two less roots}\left\{\begin{array}{l} -\sqrt[3]{11} \times : 1 + \dfrac{2c^2}{3 \cdot 6b^2} - \dfrac{2 \cdot 5 \cdot 8c^4}{3 \cdot 6 \cdot 9 \cdot 12b^4} \text{ \&c.} \\ \pm \dfrac{2\sqrt{3}}{\sqrt[3]{121}} \times : \dfrac{1}{3} - \dfrac{2 \cdot 5c^2}{3 \cdot 6 \cdot 9b^2} + \dfrac{2 \cdot 5 \cdot 8 \cdot 11c^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15b^4} \text{ \&c.} \end{array}\right\} \text{where } \frac{c^2}{b^2} = \frac{4}{121}$$

Here

$$\begin{array}{llr|l} A = & = & 1{\cdot}0000000 & \\ B = \dfrac{2c^2}{3 \cdot 6b^2} A & = & 36731 & C = \dfrac{5 \cdot 8c^2}{9 \cdot 12b^2} B = -{\cdot}0000450 \\ D = \dfrac{11 \cdot 14c^2}{15 \cdot 18b^2} C = & & 8 & \end{array}$$

$$\begin{array}{rr} + & 1{\cdot}0036739 \\ - & 0{\cdot}0000450 \\ \hline & 1{\cdot}0036289 \\ & 2 \\ \hline & 2{\cdot}0072578 \end{array}$$

2·0072578 - - log. 0·3026031

$\sqrt[3]{11}$ - - - 0·3471309

the greatest root = 4·4641016 - - 0·6497340

118. Again,

118. Again,

$\alpha = \frac{1}{3}A =$	$\cdot 3333333$	$\beta = \frac{5}{9}B =$	$\cdot 0020406$
$\gamma = \frac{11}{15}C =$	330	$\delta = \frac{17}{21}D =$	7
	$+\ \cdot 3333663$		$-\ \cdot 0020413$
	$-\ \cdot 0020413$		
	$\cdot 3313250$		
	2		
	$\cdot 6626500$ - -	log.	$\bar{1}\cdot 8212842$
	$\sqrt{3}$ - - -		$0\cdot 2385606$
	$\sqrt[3]{121}$ - - -		$-\ 0\cdot 6942618$
the latter series $\pm$	$\cdot 2320508$ - -		$\bar{1}\cdot 3655830$
$\frac{1}{2}$ the first $-$	$2\cdot 2320508$		
middle root $=$	$-\ 2\cdot 4641016$		
least root $=$	$-\ 2\cdot 0000000$		

119. But, by Art. 64, the 3 roots are -2 and $1 \pm \sqrt{12}$; hence

$$\frac{1+2\sqrt{3}}{2\sqrt[3]{11}} = 1 + \frac{2 \cdot 2^2}{3 \cdot 6 \cdot 11^2} - \frac{2 \cdot 5 \cdot 8 \cdot 2^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 11^4} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 14 \cdot 2^6}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 18 \cdot 11^6} \text{ \&c.}$$

$$\frac{2-\sqrt{3}}{4}\sqrt[3]{121} = \frac{1}{3} - \frac{2 \cdot 5 \cdot 2^2}{3 \cdot 6 \cdot 9 \cdot 11^2} + \frac{2 \cdot 5 \cdot 8 \cdot 11 \cdot 2^4}{3 \cdot 6 \cdot 9 \cdot 12 \cdot 15 \cdot 11^4} - \text{\&c.}$$

120. And in this manner the roots of cubic equations may always be found by these series; and then by comparing them with the roots of the same equations, as found by other methods, we shall obtain as many series as we please, whose sums will be given.

121. Hence

121. Hence also we may find the sum of any general series of either of these forms, namely,
$1 \mp \frac{2g^2}{3.6} - \frac{2.5.8g^4}{3.6.9.12} \mp \frac{2.5.8.11.14g^6}{3.6.9.12.15.18}$ &c. or
$\frac{1}{3} \pm \frac{2.5g^2}{3.6.9} + \frac{2.5.8.11g^4}{3.6.9.12.15} \pm \frac{2.5.8.11.14.17g^6}{3.6.9.12.15.18.21}$ &c. by comparing them with the roots of given cubic equations; whatever be the value of g, not greater than 1.

122. For, by Art. 68, $\sqrt[3]{b+c} + \sqrt[3]{b-c} = 2\sqrt[3]{b} \times :$ $1 - \frac{2c^2}{3.6b^2} - \frac{2.5.8c^4}{3.6.9.12b^4}$ &c. is = the greatest root of the cubic equation $x^3 - 3\sqrt[3]{b^2-c^2} . x = 2b$. Now make $2\sqrt[3]{b} = 1$, and $\frac{c^2}{b^2} = g^2$; so shall the above become
$\frac{1}{2}\sqrt[3]{1+g} + \frac{1}{2}\sqrt[3]{1-g} = 1 - \frac{2g^2}{3.6} - \frac{2.5.8g^4}{3.6.9.12}$ &c. = the greatest root of the equation $x^3 - \frac{3}{4}\sqrt[3]{1-g^2} . x = \frac{1}{4}$. And when g^2 or $\frac{c^2}{b^2}$ is negative, these become
$\frac{1}{2}\sqrt[3]{1+g\sqrt{-1}} + \frac{1}{2}\sqrt[3]{1-g\sqrt{-1}} = 1 + \frac{2g^2}{3.6} - \frac{2.5.8g^4}{3.6.9.12} +$ &c.
= the greatest root of the equation $x^3 - \frac{3}{4}\sqrt[3]{1+g^2} . x = \frac{1}{4}$.
So that in general the infinite series
$1 \mp \frac{2g^2}{3.6} - \frac{2.5.8g^4}{3.6.9.12} \mp \frac{2.5.8.11.14g^6}{3.6.9.12.15.18}$ &c. is
$= \frac{1}{2}\sqrt[3]{1+g\sqrt{\pm 1}} + \frac{1}{2}\sqrt[3]{1-g\sqrt{\pm 1}}$ = the greatest root of the equation $x^3 - \frac{3}{4}\sqrt[3]{1 \mp g^2} . x = \frac{1}{4}$. Where the upper and under signs respectively correspond to each other.

123. Again,

123. Again,

$$\sqrt[3]{c+b} - \sqrt[3]{c-b} = \frac{2b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2.5b^2}{3.6.9c^2} + \frac{2.5.8.11b^4}{3.6.9.12.15c^4} \&c.$$

is the leaſt root of the equation $x^3 + 3\sqrt[3]{c^2-b^2}.x = 2b$. Then, by taking $\frac{2b}{\sqrt[3]{c^2}} = 1$, and $\frac{b^2}{c^2} = g^2$, this becomes

$$\frac{\sqrt[3]{1+g} - \sqrt[3]{1-g}}{2g} = \frac{1}{3} + \frac{2.5g^2}{3.6.9} \&c. =$$ the leaſt root of the equation $x^3 + \frac{3\sqrt[3]{1-g^2}}{4g^2}x = \frac{1}{4g^2}$. And when g^2 or c^2 is negative, this becomes

$$\frac{\sqrt[3]{1+g\sqrt{-1}} - \sqrt[3]{1-g\sqrt{-1}}}{2g\sqrt{-1}} = \frac{1}{3} - \frac{2.5g^2}{3.6.9} + \&c. =$$ the leaſt root of the equation $x^3 - \frac{3\sqrt[3]{1+g^2}}{4g^2}x = \frac{-1}{4g^2}$. So that in general the infinite ſeries

$$\frac{1}{3} \pm \frac{2.5g^2}{3.6.9} + \frac{2.5.8.11g^4}{3.6.9.12.15} \pm \frac{2.5.8.11.14.17g^6}{3.6.9.12.15.18.21} \&c. \text{ is}$$

$$= \frac{\sqrt[3]{1+g\sqrt{\pm 1}} - \sqrt[3]{1-g\sqrt{\pm 1}}}{2g\sqrt{\pm 1}} =$$ the leaſt root of the equation $x^3 \pm \frac{3\sqrt[3]{1 \mp g^2}}{4g^2}x = \frac{\pm 1}{4g^2}$.

Of the Roots by another Claſs of Series.

124. But there are yet other ſeries, converging much faſter than thoſe in the foregoing claſs, by the help of which, and CARDAN's rule conjointly, may always be found

found the roots of thofe equations in which that rule fails when it is applied fingly, that is, in what is called the irreducible cafe, or that in which c^2 is negative. And thofe feries are found by introducing another cubic equation having the fame values of b and c^2 as the given equation, except that in the new equation the value of c^2 is pofitive, while in the given one it is negative. For when c^2 is pofitive, the new equation to which it belongs has only one real root, and that root is always found by CARDAN's rule; but the contrary takes place when c^2 is negative, the equation having then three real roots, although they are not always determinable by that rule, becaufe the radical quantities can feldom be extracted, on account of the fquare root of the negative quantity which is contained in them.

125. Now the general expreffion for the root by CARDAN's rule being $s + d = \sqrt[3]{b + \sqrt{\pm c^2}} + \sqrt[3]{b - \sqrt{\pm c^2}}$ or $\sqrt[3]{\sqrt{\pm c^2} + b} - \sqrt[3]{\sqrt{\pm c^2} - b}$, if the cubic roots of each of thefe be extracted by the binomial theorem, as at Art. 68, we fhall obtain thefe 4 forms;

1. $\sqrt[3]{b + \sqrt{+c^2}} + \sqrt[3]{b - \sqrt{+c^2}} = 2\sqrt[3]{b} \times : 1 - \frac{2c^2}{3 \cdot 6b^2} - \&c.$

2. $\sqrt[3]{b + \sqrt{-c^2}} + \sqrt[3]{b - \sqrt{-c^2}} = 2\sqrt[3]{b} \times : 1 + \frac{2c^2}{3 \cdot 6b^2} - \&c.$

3. $\sqrt[3]{\sqrt{+c^2} + b} - \sqrt[3]{\sqrt{+c^2} - b} = \frac{2b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2 \cdot 5b^3}{3 \cdot 6 \cdot 9c^2} + \&c.$

4. $\sqrt[3]{\sqrt{-c^2} + b} - \sqrt[3]{\sqrt{-c^2} - b} = \frac{2b}{\sqrt[3]{c^2}} \times : -\frac{1}{3} + \frac{2 \cdot 5b^2}{3 \cdot 6 \cdot 9c^2} - \&c.$

126. Of

126. Of which the series in the first and third denote the only real root of the equation when c^2 is positive, according as c is greater or less than b, which root call X; and the series in the second and fourth forms denote the greatest and least roots of the equation when c^2 is negative, which roots call R and r respectively. Then by adding and subtracting the first and second, as also the third and fourth, there result these four equations;

$$\text{R} + \text{X} = 4\sqrt[3]{b} \times : 1 - \frac{2.5.8c^4}{3.6.9.12b^4} - \frac{2.5.8.11.14.17.20c^8}{3.6.9.12.15.18.21.24b^8} \&c.$$

$$\text{R} - \text{X} = 4\sqrt[3]{b} \times : \frac{2c^2}{3.6b^2} + \frac{2.5.8.11.14c^6}{3.6.9.12.15.18b^6} + \&c.$$

$$\text{X} - r = \frac{4b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2.5.8.11b^4}{3.6.9.12.15c^4} + \&c.$$

$$\text{X} + r = \frac{4b}{\sqrt[3]{c^2}} \times : \frac{2.5b^2}{3.6.9c^2} + \frac{2.5.8.11.14.17b^6}{3.6.9.12.15.18.21c^6} + \&c.$$

127. And hence, by equal addition or subtraction, we find these two different expressions both for the greatest and least roots of a cubic equation in which c^2 or $b^2 + a^3$ is negative, namely,

$$\text{R} = -\text{X} + 4\sqrt[3]{b} \times : 1 - \frac{2.5.8c^4}{3.6.9.12b^4} - \frac{2.5.8.11.14.17.20c^8}{3.6.9.12.15.18.21.24b^8} \&c. \text{ or}$$

$$\text{R} = \text{X} + 4\sqrt[3]{b} \times : \frac{2c^2}{3.6b^2} + \frac{2.5.8.11.14c^6}{3.6.9.12.15.18b^6} + \&c.$$

$$r = \text{X} - \frac{4b}{\sqrt[3]{c^2}} \times : \frac{1}{3} + \frac{2.5.8.11b^4}{3.6.9.12.15c^4} + \frac{2.5.8.11.14.17.20.23b^8}{3.6.9.12.15.18.21.24.27c^8} \&c. \text{ or}$$

$$r = -\text{X} + \frac{4b}{\sqrt[3]{c^2}} \times : \frac{2.5b^2}{3.6.9c^2} + \frac{2.5.8.11.14.17b^6}{3.6.9.12.15.18.21c^6} + \&c.$$

where R is the greatest, and r the least root of the equa-

I tion

tion $x^3 - 3ax = 2b$ or $x^3 - 3\sqrt[3]{c^2+b^2} \,.\, x = 2b$, and x the only real root of the equation $x^3 + 3\sqrt[3]{c^2-b^2} \,.\, x = 2b$; in which, as well as in the above ſeries, c^2 denotes a poſitive quantity.

128. And hence it can no longer be ſaid that CARDAN's rule is of no uſe in the ſolution of cubic equations that have three real roots; ſince they have here been reduced to the other caſe in which the equation has but one real root, which caſe is always reſolvable by that rule. And the firſt hint of ſuch reduction I received from FRANCIS MASERES, Eſq. Curſitor Baron of the Exchequer, he having done me the favour to communicate to me the ſecond of the above four forms for the greateſt root, in a letter of the 17th of July 1779; the inveſtigation of which formula, together with thoſe of the other three, nearly as above, I had the honour of ſending him in a letter of the 26th of the ſame month; and that learned gentleman has ſince communicated to the Royal Society his ſaid formula, together with his own inveſtigation of it, done in his uſual very accurate manner. Since that time I have ſeen, in the *Memoires de l' Acad.* for the year 1743, four expreſſions ſimilar to the above, given by Mr. NICOLE for the purpoſe of ſumming certain terms of a binomial raiſed to

any power, but unaccompanied with any appearance of the idea of thus reducing the one caſe of the cubic equation to the other.

129. It is hardly neceſſary to remark, that any general ſeries of each of the above four forms, is ſummed by means of the ſum or difference of the roots of theſe two equations $x^3 - 3\sqrt[3]{b^2 \pm c^2} . x = 2b$, and that by ſubſtituting particular numbers for b and c, we may thus ſum as many ſeries of thoſe forms as we pleaſe.

130. Ex. 1. We may now illuſtrate theſe formulas by ſome examples. And firſt in the equation $x^3 - 15x = 4$. Here $2b = 4$, and $3\sqrt[3]{b^2 + c^2} = 15$, conſequently $b = 2$, and $c^2 = 5^3 - b^2 = 125 - 4 = 121 = 11^2$, and $x = \sqrt[3]{c+b} - \sqrt[3]{c-b} = \sqrt[3]{13} - \sqrt[3]{9} = \cdot 2712508$ the root of the equation $x^3 - 3\sqrt[3]{b^2 - c^2} . x = 2b$ or $x^3 + 3\sqrt[3]{117} . x = 4$. And as b is leſs than c, this equation belongs to the two ſeries in the latter caſe for finding the leaſt root. Hence, the terms of the two ſeries agreeing with the poſitive and negative terms of the ſeries in Art. 106, they will ſtand thus:

By

By the 1st series		By the 2d series	
$A = \cdot 3333333$		$B = \cdot 0020406$	
$C = \cdot 0000330$		$D = \cdot 0000007$	
$\cdot 3333663$ - log.	$\bar{1}\cdot 5229217$	$\cdot 0020413$ - - log.	$\bar{3}\cdot 3099068$
$\frac{4b}{\sqrt[3]{c^2}} = \sqrt[3]{\frac{512}{121}}$ -	$0\cdot 2088282$	$\frac{4b}{\sqrt[3]{c^2}} = \sqrt[3]{\frac{512}{121}}$ - -	$0\cdot 2088282$
series $= - \cdot 5392000$	$\bar{1}\cdot 7317499$	series $= + \cdot 0033016$ -	$\bar{3}\cdot 5187350$
$X = + \cdot 2712508$		$X = - \cdot 2712508$	
$r = - \cdot 2679492$ the least root		$r = - \cdot 2679492$ the same root.	

Agreeing with the same root found in Ex. 4. Art. 106.

131. But the same root has been found to be $-2+\sqrt{3}$ in Art. 59, and hence we obtain the sums of these two particular series, thus,

$$\frac{\sqrt[3]{13}-\sqrt[3]{9}+2-\sqrt{3}}{8}\sqrt[3]{121} = \frac{1}{3} + \frac{2.5.8.11.2^4}{3.6.9.12.15.11^4} + \&c.$$

$$\frac{\sqrt[3]{13}-\sqrt[3]{9}-2+\sqrt{3}}{8}\sqrt[3]{121} = \frac{2.5.2^2}{3.6.9.11^2} + \frac{2.5.8.11.14.17.2^6}{3.6.9.12.15.18.21.11^6} \&c.$$

132. Also by taking the sum and difference of these two, we have

$$\frac{\sqrt[3]{13}-\sqrt[3]{9}}{4}\sqrt[3]{121} = \frac{1}{3} + \frac{2.5.2^2}{3.6.9.11^2} + \frac{2.5.8.11.2^4}{3.6.9.12.15.11^4} + \&c.$$

$$\frac{2-\sqrt{3}}{4}\sqrt[3]{121} = \frac{1}{3} - \frac{2.5.2^2}{3.6.9.11^2} + \frac{2.5.8.11.2^4}{3.6.9.12.15.11^4} - \&c.$$

And this last expression agrees with what was found in Art. 108.

133. Ex. 2. Again in the equation $x^3 - 9x = -10$, we have $2b = -10$, and $3\sqrt[3]{b^2 + c^2} = 9$; consequently $b = -5$, and $c^2 = 3^3 - b^2 = 27 - 25 = 2$, which being less than b^2 or 25, this equation belongs to the first class of series, or that for the greatest root. Now

$$x = \sqrt[3]{b+c} + \sqrt[3]{b-c} = \sqrt[3]{-5+\sqrt{2}} + \sqrt[3]{-5-\sqrt{2}}$$
$$= -\sqrt[3]{5-\sqrt{2}} - \sqrt[3]{5+\sqrt{2}}$$
$$= -\sqrt[3]{3{\cdot}58578864} - \sqrt[3]{6{\cdot}41421356}$$

$= - 1{\cdot}530600 - 1{\cdot}858009 = - 3{\cdot}388609 =$ the root of the equation $x^3 - 3\sqrt[3]{b^2 - c^2}.x = 2b$, or $x^3 - 3\sqrt[3]{21}.x = -10$. And the terms of the two series are found as in Art. 110, namely $1 - \frac{2.5.8c^4}{3.6.9.12b^4} -$ &c. $= A - C - E -$ &c. $= {\cdot}9997359$, and $\frac{2c^2}{3.6b^2} +$ &c. $= B + D +$ &c. $= {\cdot}0089009$. Also $4\sqrt[3]{b} = 4\sqrt[3]{-5} = -4\sqrt[3]{5} = -\sqrt[3]{320}$. Then

By the 1st series			By the 2d series		
	·9997359	log. $\bar{1}{\cdot}9998854$		·0089009	log. $\bar{3}{\cdot}9494339$
	$-\sqrt[3]{320}$	0·8350500		$-\sqrt[3]{320}$	0·8350500
series	$= -$ 6·838098	0·8349354	series	$= -$ ·060881	$\bar{2}{\cdot}7844839$
X	$= +$ 3·388609		X	$= -$ 3·388609	
	$-$ 3·449489 the greatest root			$-$ 3·449490 the same root.	

And these values of the greatest root are nearly the same with that found in Art. 110.

134. But

134. But in Art. 61, the ſame root was found to be $-1-\sqrt{6}$, hence we obtain the ſums of theſe firſt two particular ſeries; and by the addition and ſubtraction of theſe two ariſe the other two following them, namely,

$$\frac{1+\sqrt{6}+\sqrt[3]{5+\sqrt{2}}+\sqrt[3]{5-\sqrt{2}}}{4\sqrt[3]{5}} = 1 - \frac{2.5.8.2^2}{3.6.9.12.5^4} - \&c.$$

$$\frac{1+\sqrt{6}-\sqrt[3]{5+\sqrt{2}}-\sqrt[3]{5-\sqrt{2}}}{4\sqrt[3]{5}} = \frac{2.2}{3.6.5^2} + \frac{2.5.8.11.14.2^3}{3.6.9.12.15.18.5^6} + \&c.$$

$$\frac{1+\sqrt{6}}{2\sqrt[3]{5}} = 1 + \frac{2.2}{3.6.5^2} - \frac{2.5.8.2^2}{3.6.9.12.5^4} + \frac{2.5.8.11.14.2^3}{3.6.9.12.15.18.5^6} - \&c.$$

$$\frac{\sqrt[3]{5+\sqrt{2}}+\sqrt[3]{5-\sqrt{2}}}{2\sqrt[3]{5}} = 1 - \frac{2.2}{3.6.5^2} - \frac{2.5.8.2^2}{3.6.9.12.5^4} - \&c.$$

And the laſt but one of theſe equations agrees with one found in Art. 112.

135. Ex. 3. Alſo in the equation $x^3-12x=9$, we have $2b=9$, and $\sqrt[3]{b^2+c^2}=4$; conſequently $b=\frac{9}{2}$, and $c^2=4^3-b^2=64-\frac{81}{4}=\frac{175}{4}$, which being greater than b^2 or $\frac{81}{4}$, this caſe belongs to the ſecond claſs of ſeries, or that of the leaſt roots. Now here $x=\sqrt[3]{c+b}-\sqrt[3]{c-b}=\sqrt[3]{\frac{\sqrt{175}+9}{2}}-\sqrt[3]{\frac{\sqrt{175}-9}{2}}=\sqrt[3]{11{\cdot}114378}-\sqrt[3]{2{\cdot}114378}=2{\cdot}2316619-1{\cdot}2834950={\cdot}9481669=$ the root of the equation $x^3-3\sqrt[3]{b^2-c^2}\,.\,x=2b$ or $x^3+3\sqrt[3]{\frac{47}{2}}\,.\,x=9$. And the terms of the two ſeries being found as in Art. 113, namely, A + C + E + &c. $= {\cdot}34051$, and B + D + F + &c. $= {\cdot}03071$, alſo $\frac{4b}{\sqrt[3]{c^2}}$ being $= \frac{36}{\sqrt[3]{250}}$, we ſhall have

By

By the 1st series		By the latter series	
·34051 - log.	$\bar{1}$·5321299	·03071 - log.	$\bar{2}$·4871798
$\frac{36}{\sqrt[3]{350}}$ - -	0·7082798	$\frac{36}{\sqrt[3]{350}}$ - -	0·7082798
series = − 1·739441 -	0·2404097	series = + ·1568771 -	$\bar{1}$·1955596
X = + 0·948167		X = − ·9481669	
− ·791274 the least root		− ·7912898 the same root	

Which nearly agree with the same root found in Art. 113.

136. But in Art. 62 the same root was found to be $\frac{3-\sqrt{21}}{2}$, hence then we shall have these first two following equations, and by means of their sum and difference we obtain the other two:

$$\frac{\sqrt[3]{20\sqrt{7}+36}-\sqrt[3]{20\sqrt{7}-36}+\sqrt{21}-3}{72}\sqrt[3]{350}=\frac{1}{3}+\frac{2.5.8.11.81^2}{3.6.9.12.15.175^2}\ \&c.$$

$$\frac{\sqrt[3]{20\sqrt{7}+36}-\sqrt[3]{20\sqrt{7}-36}-\sqrt{21}+3}{72}\sqrt[3]{350}=\frac{2.5.81}{3.6.9.175}+\&c.$$

$$\frac{\sqrt[3]{20\sqrt{7}+36}-\sqrt[3]{20\sqrt{7}-36}}{36}\sqrt[3]{350}=\frac{1}{3}+\frac{2.5.81}{3.6.9.175}+\&c.$$

$$\frac{\sqrt{21}-3}{36}\sqrt[3]{350}=\frac{1}{3}-\frac{2.5.81}{3.6.9.175}+\frac{2.5.8.11.81^2}{3.6.9.12.15.175^2}-\&c.$$

And the last of these agrees with one found in Art. 115.

137. Ex. 4. In the equation $x^3-15x=22$, we have $2b=22$, and $\sqrt[3]{b^2+c^2}=5$; consequently $b=11$, and $c^2=5^3-b^2=125-121=4$, which being less than b^2 or 121, this belongs to the first class of series, or that for the greatest root.

Now

Now $x = \sqrt[3]{b+c} + \sqrt[3]{b-c} = \sqrt[3]{13} + \sqrt[3]{9} = 4{\cdot}4314186 =$ the root of the equation $x^3 - 3\sqrt[3]{117}.x = 22$. And the terms of the two series being found as in Art. 117, we have the first $= A - C - \&c. = 1 - {\cdot}0000450 = {\cdot}9999550$, and the second $= B + D + \&c. = {\cdot}0036731 + {\cdot}0000008 = {\cdot}0036739$. Also $4\sqrt[3]{b} = 4\sqrt[3]{11} = \sqrt[3]{704}$. Hence,

By the 1st series			By the 2d series		
	$\cdot 9999550$	log. $\bar{1}{\cdot}9999805$		$\cdot 0036739$	log. $\bar{3}{\cdot}5651273$
	$\sqrt[3]{704}$	$0{\cdot}9491909$		$\sqrt[3]{704}$	$0{\cdot}9491909$
series $=$	$+ 8{\cdot}8955200$	$0{\cdot}9491714$	series $=$	$+ {\cdot}0326827$	$\bar{2}{\cdot}5143182$
X $=$	$- 4{\cdot}4314186$		X $=$	$+ 4{\cdot}4314186$	
	$+ 4{\cdot}4641014$	greatest root		$+ 4{\cdot}4641013$	the same root

Which nearly agree with the same root found in Art. 117.

138. But in Art. 64, the same root was found to be $1 + \sqrt{12} = 1 + 2\sqrt{3}$, hence we obtain these two first equations following, and their sum and difference give the other two:

$$\frac{1+\sqrt{12}+\sqrt[3]{13}+\sqrt[3]{9}}{4\sqrt[3]{11}} = 1 - \frac{2.5.8.2^4}{3.6.9.12.11^4} - \frac{2.5.8.11.14.17.20.2^8}{3.6.9.12.15.18.21.24.11^8} - \&c.$$

$$\frac{1+\sqrt{12}-\sqrt[3]{13}-\sqrt[3]{9}}{4\sqrt[3]{11}} = \frac{2.2^2}{3.6.11^2} + \frac{2.5.8.11.14.2^6}{3.6.9.12.15.18.11^6} + \&c.$$

$$\frac{\sqrt[3]{13}+\sqrt[3]{9}}{2\sqrt[3]{11}} = 1 - \frac{2.2^2}{3.6.11^2} - \frac{2.5.8.2^4}{3.6.9.12.11^4} - \frac{2.5.8.11.14.2^6}{3.6.9.12.15.18.11^6} - - \&c.$$

$$\frac{1+\sqrt{12}}{2\sqrt[3]{11}} = 1 + \frac{2.2^2}{3.6.11^2} - \frac{2.5.8.2^4}{3.6.9.12.11^4} + \frac{2.5.8.11.14.2^6}{3.6.9.12.15.18.11^6} - + \&c.$$

The last of which agrees with one found in Art. 119.

And thus we may find the ſums of as many ſeries of theſe kinds as we pleaſe; as well as the ſum of any of the general ſeries, by means of the roots of given cubic equations. As to the ſummation of other forms of ſeries by means of the roots of equations of other orders, I ſhall perhaps treat of them on ſome future occaſion.

XXVI. *An Account of a most extraordinary Degree of Cold at* Glasgow *in* January *last; together with some new Experiments and Observations on the comparative Temperature of Hoar-frost and the Air near to it, made at the* Macfarlane *Observatory belonging to the College. In a Letter from* Patrick Wilson, *M. A. to the Rev.* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read April 20, 1780.

S I R,

Glasgow College,
Feb. 27, 1780.

THE observations and experiments related in the sequel were made here during the great frost in the course of last month. I shall be extremely happy if they contribute any thing to your entertainment, being induced to send you the account in consequence of your having on a former occasion so politely invited me to a further correspondence. If the paper shall appear to

you deserving the notice of the Royal Society, you will please to consider it as entirely at your own disposal.

Most part of the time my father was confined to his room with bad health, and could not therefore gratify his curiosity by taking a part in the observations. I have now the pleasure of acquainting you, that he is quite well again, and on this occasion he begs to be remembered to you with best compliments.

On Tuesday, Jan. 11th, there was a slight frost, and on the evening of that day we had a fall of snow to the depth of twelve inches. Next day the cold continued to increase, but so gradually, that at sun-set FAHRENHEIT's thermometer pointed only to 22°. Upon returning home to the College from a visit about midnight, I hung out a very accurate thermometer at a high North window, where it soon after pointed to 6°. At this time the air was very still and serene, and the barometer stood at 30 inches.

Thursday

Thurſday morning, Jan. 13, 1 o'clock, thermometer pointed to . .	gr. +6
$1\frac{3}{4}$	+6
$2\frac{1}{2}$	+4
3	+6
4	+3
$4\frac{1}{2}$	+2
5	+2
$5\frac{1}{2}$	0

At 6 o'clock this morning I carried the thermometer over to the Obſervatory Park, and when it was there laid down upon the ſnow the mercury ſunk to gr. 13 below 0. Your friend Profeſſor ANDERSON ſoon after arriving, we repeated the obſervation with an excellent thermometer made by Mr. RAMSDEN, and it agreed exactly with the other.

At this time we thought it unneceſſary to ſtay abroad ſo long in the cold as to try the temperature of the air by hanging up the thermometers, eſpecially as we imagined that this had been done more readily, and as truly, by taking the degree from the ſurface of the ſnow which had been

expofed to the open air during the night; but upon reflecting afterwards on the fnow at the Obfervatory being fo much below 0, the greateft cold of the air at the College, and having on other occafions found a difference of only 4° at moft in air at thefe two ftations, I was led into a fufpicion that the fnow might perhaps have been fo far cooled down by an evaporation at the furface. With a view to this opinion I projected the experiment with the bellows defcribed below, by which I was not without expectations of producing a ftill more remarkable fall of the thermometer when lying on the fnow. Before dinner this day I met with Dr. IRVINE, to whom I communicated the above obfervations, and the intention of trying the evaporation; for that fnow and ice did actually evaporate in fome circumftances feemed fufficiently proved by an experiment made here by my father in 1768, which is related in the Philofophical Tranfactions, vol. LXI. p. 326. Dr. IRVINE approved much of the propofal, and moft obligingly agreed to fpend the night at the Obfervatory, that we might, by a regular courfe of obfervations, afcertain the difference of temperature, and try whether evaporation was really the caufe of it.

All the afternoon the cold was very intense, and at seven o'clock at night the thermometer at the high North window pointed to 0. At eight Dr. IRVINE and I repaired to the Observatory, and made choice of a station at a sufficient distance from the house, and to the windward, as a light air was felt coming from the East. Here we laid down two thermometers on the snow with their balls half immersed, and hung up other two freely exposed to the air at two feet and a half from the surface.

In the following observations the interruption of the series from $2\frac{1}{2}$ to $6\frac{1}{2}$ o'clock was owing to an accident having befallen one of the thermometers whilst the other was employed in the trials, of which an account is subjoined.

Thursday,

Thursday evening, Jan. 13.

	Below o.	
$8\frac{1}{2}$ o'clock, therm. on the snow pointed to	gr. −12	Therm. in air to gr. 0
9 . . .	−14	. −2
10 . . .	−14	. −4
11 . . .	−17	. −6
$11\frac{1}{2}$ . . .	−18	. −6
$\frac{1}{2}$ o'clock Friday morning	−20	. −8
1 . . .	−23	. −7
$1\frac{1}{2}$ . . .	−22	. −8
2 . . .	−22	. −9
$2\frac{1}{2}$ . . .	−21	. −8
3 . . .	. .	. −9
$3\frac{1}{2}$ . . .	. .	. −10
4 . . .	. .	. −12
$4\frac{1}{2}$ . . .	. .	. −12
5 . . .	. .	. −12
$5\frac{1}{2}$ . . .	. .	. −12
6 . . .	. .	. −14
$6\frac{1}{2}$ . . .	−22	. −13
7 . . .	−22	. −13
$7\frac{1}{2}$ . . .	−22	. −13
8 . . .	−19	. −10

EX-

EXPERIMENT I.

At half paft one o'clock, when the thermometer pointed to – 22°, the fnow contiguous to the ball was blown upon for two minutes by a pair of hand-bellows, held with the pipe nearly horizontal, and half a foot above the furface of the fnow. The bellows had been lying out on the fnow to cool from the time we firft came over; and, in order to promote their cooling, they were now and then wrought in the open air. Care was alfo taken to ftand to leeward of the thermometer, and to extend the bellows as far as poffible from the body in the time of blowing. We were furprifed to find, however, notwithftanding all our precautions, that the thermometer at the end of the experiment had got up no lefs than 10°, for it now pointed only to – 12°. In this experiment the nofle of the bellows was held about fix inches from the thermometer, but the blaft, though moderate, frequently drifted away the fnow from the ball.

EXPERIMENT II.

At half paft two o'clock, a bread-bafket was filled with fnow, taken up near the ground at + 14°. The contents being relatively fo warm, the bafket was placed

to leeward of the common ſtation, and the thermometer laid on the ſurface of this ſnow.

At 3 o'clock in the morning, thermometer on the baſket pointed to . . .	−10°
$3\frac{1}{2}$	−15
4	$-16\frac{1}{2}$
$4\frac{1}{2}$	−18
5	−18
$5\frac{1}{2}$	−18
6	−18

EXPERIMENT III.

At four o'clock in the morning, when the thermometer in the baſket had got down to − 16°, a piece of thin fir plank about a foot ſquare was laid on the ſnow, upon which was placed a ſmall plate of tin which accidentally lay at hand. Upon this was laid one of the thermometers which had been hanging in the air.

At 5 o'clock, therm. on the plate pointed to	−16
$5\frac{1}{2}$	−16
6	−18

At ſeven this morning Profeſſor ANDERSON made us a viſit, and had the ſatisfaction of ſeeing the thermometers whilſt they indicated ſuch an extraordinary degree of cold.

cold. During the whole time not a cloud was perceivable, but there was a faint haze in the air when viewed towards the horizon. There was little or no tremor in the atmofphere, which made the ftars to fhine with a full and fteady light like that of the planets. Many of the citizens, who had thermometers hung out at their windows in different places of the town, found them pointing feveral degrees below o at nine o'clock in the morning. On the afternoon of this day, being Friday, Jan. 14th, the air became much warmer, and the barometer had now fallen four-tenths. Next day a thaw came on, and continued for fome time.

As the above experiment with the bellows favoured fo little the opinion, that the difference of temperature was caufed by evaporation, I began to think, that it might be owing to a continued defcent of cold air fomewhere in the neighbourhood, which cooled the fnow by fweeping along its furface in fo thin a fheet as not to affect the air a little higher up. But as this folution of the phenomenon was fo arbitrary, and unfupported by any facts, I wifhed for another opportunity of making further experiments, and of enquiring into circumftances ftill more attentively. A good occafion offered on Saturday, Jan. 22. The froft, which before this time had again returned, became on this night very keen;

 and

and a good deal of the former snow yet remaining on the ground, the following observations and experiments were made whilst I was favoured with the company and assistance of Dr. ADAIR CRAWFORD, who passed the whole of this night at the Observatory.

On Sunday morning, Jan 23,

½ o'clock, therm. on the snow pointed to	+4	Therm. in air +14
¾ . .	+5	. +14
1¼ . .	+4	. +11
1¾ . .	+3	. +15
2¼ . .	+3	. +12
2¾ . .	+3	. +11
3½ . .	+1	. + 8
4 . .	+1	. + 6
4½ . .	0	. + 6
5 . .	−1	. + 5
5½ . .	−1	. + 6
6¼ . .	−1	. + 6
7 . .	0	. + 6
7½ . .	−3	. + 5
7¾ . .	−2	. + 5
8½ . .	+1	. + 7

EX-

EXPERIMENT IV.

This night, inftead of blowing on the fnow, we fanned it by means of a fheet of brown paper fitted to the end of a long flender ftick. This apparatus was previoufly cooled by lying on the fnow, and in fanning we took care to ftand to leeward of the thermometer. The effect was, that the mercury rofe nearly to the fame degree given by the thermometer in air at the fame time.

EXPERIMENT V.

At three quarters paft one o'clock, when the thermometer on the fnow pointed to + 3°, it was fcreened by two fheets of brown paper fet up on their edges, and fo inclined againft one another as to ftand. The paper had been previoufly cooled by lying on the fnow. At a quarter paft two o'clock the thermometer thus fheltered pointed to +9°. This experiment was afterwards repeated with the fame event.

EXPERIMENT VI.

We next went up to the leads of the Eaſt wing of the Obſervatory. Here we hung a thermometer to the hook of a long pole, and raiſed it in the air about twenty-four feet from the ground, and at the ſame time inclined the pole over the balluſtrade, ſo as to put the inſtrument fully to windward of the houſe.

Upon ſuddenly lowering the pole, after half an hour, and examining the thermometer, the air at that elevation was found to be pretty conſtantly 4° warmer than at the ſtation below.

EXPERIMENT VII.

The reſult of this trial appeared more remarkable than any thing which had hitherto occurred. We lowered the pole till the thermometer was brought down within half a foot of the balluſtrade, but keeping it ſtill a few inches to windward of the building, and by this means it was found that the air here was never colder than +10°. Upon the balluſtrade there happened to be

 ſeveral

feveral detached bodies which had attracted a very thick hoar-froft. When the thermometer was taken off the hook of the pole, and laid on this hoar-froft, there was always a remarkable fall of the mercury, not lefs than 6°. Both Dr. CRAWFORD and I were much ftruck with this fact, and attended to it very carefully.

In tranflating the inftrument from the pole to the balluftrade, it was commonly laid on fome hoar-froft three quarters of an inch deep, which had fettled on a piece of thin board which had been for years expofed to the weather. Some fragments of the hoar-froft were alfo made to touch the upper part of the ball; which was done by pufhing them on with a long frozen ftraw.

EXPERIMENT VIII.

When the thermometer taken from the pole as in laft experiment was laid on pieces of ftone, from which the hoar-froft had been brufhed away for fome time before, the mercury funk but very little by fuch a change of fituation.

Next night, being that of Sunday Jan. 23. the thermometers were placed in their former ftation below, when at

9 o'clock

9 o'clock at night, therm. on ſnow pointed on	+5	Therm. in air to	+9
$9\frac{1}{2}$ . .	+5	.	+ 8
10 . .	+6	.	+ 8
$10\frac{1}{2}$ . .	+6	.	+10
11 . .	+6	.	+ 9
$11\frac{1}{2}$ . .	+5	.	+ 8
12 . .	+5	.	+ 8
$12\frac{1}{2}$ . .	+4	.	+ 7
1 o'clock Monday morning	+4	.	+ 8
$1\frac{1}{2}$ . .	+4	.	+ 8

From theſe obſervations it appears, that the cold now was very moderate when compared to that of the 14th, and ſomewhat more moderate than that of the preceding night. Experiment 7th was again repeated with a ſimilar reſult, though the difference of temperature was not now ſo great.

This night I made another experiment with a view to the evaporation, not ſo liable to objections as thoſe of the bellows and the fan. This ſhall now be deſcribed.

EXPERIMENT IX.

When the thermometer in air at the lower ſtation had contracted a conſiderable film of frozen matter all over

over the ball, it was ſwung round at the end of a pack-thread, about a yard and a half long. Upon ſtopping the motion at the expiration of two minutes, and making the ſervant who waited approach quickly with a lighted candle, I found the mercury had got up 2°. In this experiment, which was repeated four times with the ſame reſult, care was always taken to keep the inſtrument to windward of our bodies, and of the lighted candle.

This night the thermometer, when put under the ſnow cloſe to the ground, pointed to +18°. Both now and on the preceding night there were no clouds, though the air, as on the 14th, was a little thick towards the horizon, and there was always a very perceptible and conſtant motion of it from Eaſt to Weſt. On this and the preceding night the Moon, a little paſt the full, ſhone very bright, and the mercury was ſtationary at 30 inches.

On Sunday, Feb. 13. we had a ſlight froſt accompanied with a thick fog. This occaſioned a vaſt ſettling of hoar-froſt upon the ſmall branches and twigs of trees, and upon all thin and detached bodies expoſed to the open air. The weather continued much in this ſtate on Monday, when, at ten o'clock forenoon, I repeated the ſeventh experiment on the balluſtrade, but now found no difference whatever in the temperature of the hoar-froſt and the air in its neighbourhood. The thermome-

ter in both caſes pointed to +22°. About 11 o'clock the ſun-ſhine broke through the fog, when the temperature of the hoar-froſt was as quickly affected as that of the air. The air on this day was quite ſtill, and equally cold both at the upper and under ſtations. It may alſo be here obſerved, that no ſnow lay on the ground. The ninth experiment was alſo repeated, but the thermometer was not now in the leaſt affected by ſwinging it round. Neither on this occaſion, nor when the experiment was firſt made, did any of the frozen matter appear to have parted from the ball.

If we conſider the exceſs of cold in the ſnow which Dr. IRVINE and I firſt obſerved as a phenomenon of the ſame kind with that deſcribed in the ſeventh experiment, and proceeding from the ſame cauſe, it is manifeſt that neither the one nor the other can be accounted for by any previous cold ſtate of the air, according to an hypotheſis alluded to in the beginning of this letter: for in the ſeventh experiment the air at the balluſtrade was never colder than +10°, and yet the hoar-froſt there was at the ſame time found in ſeveral inſtances as cold as +2°. That both theſe phenomena are of the ſame kind appears extremely probable from this conſideration, namely, that when the ſnow upon the fields was attentively examined, the ſurface was found quite covered over

over with the fame fort of hoar-froft which was attached to other bodies which had been long expofed.

The two following experiments afford fome grounds for believing that no kind of evaporation was going on at the time the remarkable excefs of cold in the fnow and hoar-froft was obferved.

EXPERIMENT X.

On Sunday morning, Jan. 23. before one o'clock, Dr. CRAWFORD and I repeated the experiment with the metal fpeculum which was tried here in 1768. A large fpare metal of a two-foot telefcope was laid out to cool, after which a film of ice was imparted to its polifhed furface by breathing on it four or five times. It was then expofed as before, and in half an hour the whole film difappeared in the way of evaporation. But when the experiment was again repeated, and a thicker film imparted, fome of this, towards the middle of the fpeculum, remained fixed, and would not go off after long expofure. The fpeculum was next warmed, and its polifhed furface made quite clean, and then laid out for two hours and a half. Before the expiration of this time it began to draw frozen matter from the air, which fettled all over the polifhed furface in long parallel lines which

 gradually

gradually multiplied, till at length it was moftly covered with a thin film refembling a fpider's web.

The evaporation fhewn in the firft part of this experiment was probably owing to the fpeculum not having been fufficiently cooled when the film was firft communicated to it from the lungs, and to its being further heated by that very operation. In the fecond part of the experiment the evaporation feems to have ftopped when the heat in the metal which favoured the procefs was exhaufted; that is, when the fpeculum had arrived at the temperature of the ambient air, for after that no heat could pafs from the metal in order to contribute to the evaporation. But from the laft part of the experiment, the true difpofition of the air at that time, relative to bodies as cold or colder than itfelf, feems to be determined, namely, that of giving out or depofiting hoar-froft.

EXPERIMENT XI.

On Sunday night, January 23, feveral things were laid out at the Obfervatory, fuch as fheets of brown paper, pieces of boards, plates of metal, glaffes of feveral kinds, &c. which all began to contract hoar-froft feemingly as foon as each body had time to cool down

down to the temperature of the air. The ſheets of brown paper being ſo thin acquired it ſooneſt, and when beheld in candle-light they became beautifully ſpangled over by innumerable reflections from the ſmall cryſtals of hoar-froſt which had parted from the air.

Evident ſymptoms of the ſame tendency of the air to depoſit occurred on all the former nights of obſerving, by which the tubes of the thermometers were ſo much ſtained, that it required ſome attention to keep that part which correſponded to the ſcale quite clear.

Theſe experiments indeed rather favour the opinion of the exceſs of cold at preſent treated of depending upon a principle the very reverſe of evaporation. But till opportunities offer in this or in a colder climate of making more experiments, it will be too early to ſay any thing deciſive concerning the nature or extent of a COOLING PROCESS which has ſo recently come under obſervation. All that can at preſent be affirmed is, that in certain circumſtances ſuch a proceſs goes on, and that it depends probably upon principles different from evaporation or chymical ſolution. At the ſame time ſome of the experiments ſhew that a free communication betwixt the hoar-froſt and external air, perhaps whilſt in motion, is neceſſary; but in what manner this promotes the REFRIGERATION doth not as yet appear.

 Thoſe

Thoſe gentlemen whoſe opportunities have led them to know how far the philoſophy of heat hath, of late times, been extended by the great diſcoveries of Dr. BLACK, Dr. IRVINE, and Dr. CRAWFORD, will be moſt intereſted in the views which offer from the foregoing experiments. If at firſt ſight there appears any thing adverſe to certain general principles already eſtabliſhed, yet we may reſt ſatisfied that this is but in appearance only; as the perfect method of induction, purſued by theſe philoſophers, leaves no room to apprehend that any future diſcovery will militate againſt their concluſions. In the further proſecution of this ſubject, and in whatever way it may be cleared up, it is probable, that we ſhall meet with a fine inſtancc of the congruity of nature in all her operations, and of the ſtability of thoſe general laws, which have been derived from a cautious obſervance of the rules of the experimental philoſophy.

I am, &c.

The following obſervations which relate to the diſpoſition of the air in giving out hoar-froſt may be here ſubjoined.

It would be going too far were we to conclude from the experiments related above, " that very cold air is " never

"never difpofed to depofit its contents except upon bo-
" dies as cold or colder than itfelf." And yet that this is frequently the cafe feems probable from a number of common appearances. We often find, after a night of froft, the flates and other thinner parts about a houfe whitened with hoar-froft, when the walls and more folid parts of the building remain quite free. In like manner the fmaller branches and twigs of trees often acquire this frozen ornament, when the main branches and trunk remain naked for a long time; and, in general, any thin or detached body, capable of being eafily cooled, attach hoar-froft the fooneft.

In favour of this general pofition, the following remarkable cafe lately occurred, which at the time I fhewed to Dr. REID, in confequence of whofe approbation I am induced at prefent to bring it into view.

Between the public library and the buildings of the new court there is a long rail compofed of bars of caft iron, but divided into two parts by two maffy ftone pillars which fupport the iron gate-way that leads into the garden. The bars are about fix feet high and an inch fquare, and faftened with lead into a ftone parapet below in the ufual way. A few bars much larger are fet in among the reft at regular diftances, in order to give the rail more ftability. On Sunday morning, Feb. 13, when we

we had the ſlight froſt accompanied with a fog, it was entertaining to obſerve how the hoar-froſt had ſettled during the night upon theſe bars. Very little was to be ſeen upon the flat ſides, but a great deal upon the angles, by which means from the top downward every bar was garniſhed with four fringes, which made the whole rail look very gay and ornamental. Running the eye along the foot of the bars near to the parapet it was obſerved, that the fringe of hoar-froſt upon the corners ſtopped ſhort about twelve inches from the bottom, and that ſo much of every bar was entirely free. Two bars next the houſe and two next the library were likewiſe perfectly clear of it from top to bottom. One bar next the pillar of the gate was quite free, and the ſecond had contracted but little. The ſame thing preciſely may be ſaid of the two bars contiguous to the other pillar. And it was alſo obſerved, that the few thicker and ſtronger bars were leſs fringed at the corners, and were quite free much farther above the parapet than the others.

It is manifeſt, that during the night the air ſurrounding the bars muſt have been conſtantly endeavouring to make them as cold as itſelf, whilſt they, on the other hand, reſiſted this change by drawing heat from every neighbouring ſource which offered it, namely, from the parapet, from the pillars in the middle, and from the pillars at

at both ends immediately adjoining to the library, and to the houfe in the new court; for thefe bodies from their great bulk muft have been but very little cooled in the courfe of the night. Wherever the air feems to have got the better in this ftruggle, as at the angles of the bars, which evidently muft be the parts the fooneft cooled, there we find that the hoar-froft was depofited, but no where elfe.

Several other inftances were found quite of the fame kind with that of the rail. Among the reft, a figure of an unicorn in ftone, which ftands within the college, had refifted the attacks of the air all to the tip of his horn, which accordingly was the only part diftinguifhed by a patch of hoar-froft. Befides this kind of hoar-froft which joined itfelf to bodies by a regular arrangement, there was fome of a different fort found upon the uppermoft furface of fuch bodies as were fully expofed to the open air. But this always lay fcattered like very thin flakes of meal, or hair-powder, and was found to proceed from minute parts, moftly columnar, previoufly formed in the air, falling down by their own gravity.

XXXVI. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1779. *By* Thomas Barker, *Esquire. Communicated by* Thomas White, *Esquire, F. R. S.*

Read May 11, 1779.

		Barometer.			Thermometer.						Rain.
					In the House.			Abroad.			
		Highest	Lowest.	Mean.	High.	Low.	Mean	High.	Low.	Mean	
Jan.	Morn.	30,13	29,22	29,81	44½	34	38½	44	25	34	0,212
	Aftern.				45	35	39	49½	29	38	
Feb.	Morn.	30,06	29,26	29,77	49	41	46	44	29	40	2,239
	Aftern.				52	42½	47	55½	40	49	
Mar.	Morn.	30,13	29,18	29,73	53	42½	47	46	26½	38	0,131
	Aftern.				55	44	48	60	42	50½	
Apr.	Morn.	30,07	28,94	29,49	56½	45	51	52	32½	43½	1,888
	Aftern.				63½	48½	53	72½	47	55	
May	Morn.	29,84	29,14	29,44	64	45½	54	59	36	48½	1,266
	Aftern.				65½	46	55½	76	45½	59½	
June	Morn.	29,82	29,00	29,52	62	55	58½	60½	46½	53	2,416
	Aftern.				63½	56	60	74	59	63½	
July	Morn.	29,94	28,87	29,47	73½	60	65	66½	53	59	4,036
	Aftern.				75½	61	67	81½	60½	72	
Aug.	Morn.	29,89	29,24	29,62	70½	61	64½	65	49	58	1,508
	Aftern.				75	62	67	81½	61	72	
Sept.	Morn.	29,71	29,05	29,42	68	55	61	60½	42½	53	1,227
	Aftern.				69	57	62½	73	55	65	
Oct.	Morn.	29,97	28,89	29,50	60	50½	54	55	34	45½	1,769
	Aftern.				59	52½	55½	64	47	56	
Nov.	Morn.	29,86	28,24	29,19	54	36	44½	50	24½	38	2,050
	Aftern.				54	36½	45	57½	31½	43½	
Dec.	Morn.	29,87	28,35	29,21	50½	30	40½	52	14½	35	3,136
	Aftern.				50	31	41	55	22½	38½	
											19,878

The end of winter and beginning of ſpring was warmer in 1779 than in 1778; the end of ſpring and beginning of ſummer 1778 was hotter than 1779; the end of ſummer and autumn 1779 hotter than in 1778; but the winter 1779 was much colder than that after 1778.

After the great ſtorm of January 1. came all the froſt there was this winter, which was not much; ſome broken froſts in the firſt nineteen days of January, after which there was ſcarce any at all. The weather was uncommonly mild, and more like ſpring than winter, and ſo dry that there was not much more than half an inch of rain in the firſt three months, and but an inch and a half in four months from Dec. 12. to Apr. 12. it was like that of the fine winters after 1742, 49, and 60, or that more remarkably dry, mild, and fine one after 1733, of which ſome account is given in the Ladies Diary for 1735; but many ſaid they never remembered ſo mild a winter: and what was remarkable, this winter, which ſeems to have been a mild one in all the North of Europe, was reckoned a ſevere one in the South of it.

February was ſo mild and fine that the wall-fruit flowered, and had better weather, and ſet much fuller, than the apples, which were two months later; the graſs grew alſo very conſiderably. The ſpring

ſeed-time was very fine, and every thing very forward, the vines put out in March, and the weather was in general fine, mild, and dry, though not without ſome cold N.E. winds, as is uſual in ſpring. The middle of April was quite hot, but the end of that month and beginning of May were cold, very ſhowery, and blaſted the apples, which were ſcarce this year; but that rain was very uſeful, for the graſs wanted it, and there was alſo a ſcarcity of water in many places; the reſt of May was hot and dry, or ſometimes hot ſun and cold winds.

It was a hot ſummer, and in general dry, yet with ſuch fits of rain as kept it from burning too much; but graſs was upon the whole rather ſcarce, and there was a great want of water in many places. The year was an uncommon one for fine weather, chiefly hot, and the fruits forwarder than uſual. Hay was not plentiful, but well got, and the bees were remarkably rich. A wet fit in wheat harveſt made ſome of it grow both cut and ſtanding, but in general it was well got, and the reſt of the harveſt fine, and all plentiful except peaſe; and grain cheaper than it has been for many years: wheat 26 or 28 ſhillings a quarter, and barley 15 or 16. The latter end of Auguſt was very hot, after which it grew gradually more moderate, but was ſtill dry and burning.

Some

Some ſhowers at the end of September and in October made the ground in good order for ſowing wheat, which came up very well, and was early. The autumn was mild and fine, and ſcarce any froſty mornings till near the middle of November, after which it was very winterly; either froſty, as it was the latter half of November, and from December 22. to February; or wet, as it was part of November, and the firſt three weeks of December. The froſts this winter were very ſevere, but never laſted long without ſome breaks.

Though the ſummer was very fine, it does not appear to have been healthy; there has been more illneſs than uſual ever ſince. In Auguſt (eſpecially in the low countries) colds were almoſt univerſal, and there were many ſore throats and fevers.

XXIX. *Journal of the Weather at* Senegambia, *during the Prevalence of a very fatal putrid Diſorder, with Remarks on that Country. By* J. P. Schotte, *M. D.*; *communicated by* Joſeph Banks, *Eſq. P. R. S.*

Read May 11, 1780.

S I R,

London,
Feb. 18, 1780.

HAVING kept a meteorological journal at the iſland St. Lewis, in the river Senegal, in Africa, during a time when the greateſt part of the garriſon, and a great number of the inhabitants on the iſland as well as on the continent, died of a putrid diſorder, I communicate the ſame to you, as I thought this fatal circumſtance a ſufficient reaſon to make it acceptable to the Society over which you preſide; hoping, that it may afford matter to determine the cauſe of it, and lead to find out remedies to prevent it in future in the like climates.

Previous to the journal, in order to illuſtrate it, I think it requiſite to make a few remarks on the ſituation of the iſland, the country about it, its ſeaſons, the manner and

 time

time in which the diſorder appeared and ceaſed, and the thermometer I have uſed.

The iſland St. Lewis, otherwiſe called Senegal, is ſituated in 16° North latitude, and 16° Weſt longitude. It is ſeparated from the iſland of Soar on the Eaſt by the main river, which, on account of the ſmallneſs of the creek by which it is formed, is eſteemed a part of the continent. It has the Atlantic Ocean on the Weſt, from which it is ſeparated by a ſmall neck of land, or more properly ſand, called Barbary Point. This neck of land is in ſeveral places not above five or ſix hundred yards broad. A branch of the river runs between it and the iſland itſelf, communicating with the main river above and below the iſland. It is about a mile in length, ſeven hundred feet in breadth, and contains five or ſix thouſand black inhabitants. In the months of Auguſt, September, and October, it is uſually about two or three feet above the level of the river at high water; but there are years in which the whole iſland is overflowed; in the other months of the year it may be about five or ſix feet above its level in the higheſt places. The continent and iſlands near it are as low, and in many places much lower, being overflowed for the moſt part during the rainy months; the latter are formed by creeks communicating with the main river, and thickly beſet with mangroves.

mangroves. The water of the river is fresh during the rains, but very thick and troubled, the current being so rapid and strong as to stop the flood-tide; but in the dry months the river water is salt, and no other water is to be had, but such as is procured by digging a pit into the sand more or less deep according to the height of the ground into which the water filtrates from all sides, and gathers up to the level of the river. This water is brackish, but as no better is to be had thereabouts, the garrison, as well as the inhabitants, make use of it, except when the river is fresh.

The year is commonly divided by the Europeans as well as the inhabitants into two seasons; *viz.* the rainy and dry; by others called the sickly and healthy season. The rainy or sickly one generally begins about the middle of July, and ends about the middle of October; during this time the wind is generally between the points of East and South, the quarter from which the tornados come. It has been observed, that this season is more or less unwholesome in proportion to the greater or lesser quantity of rain that falls. A tornado is preceded by a disagreeable closeness and weight in the air, (which seems to be much hotter than the thermometer shows it to be); and it is known to come on by the rising of the clouds to the South-east, which by joining grow darker,

darker, ſo as to make the horizon look quite black, accompanied with lightning and thunder at a diſtance. The breeze dies away by degrees as the tornado advances, and an entire calm ſucceeds; the air grows yet darker; animals and birds retire and ſhelter themſelves; every thing is ſilent, and the aſpect of the ſky, from whence the tornado approaches, is moſt dreadful. A violent ſtorm comes on all at once, which is ſo cold as to occaſion the thermometer to fall ſeven or eight degrees in a few minutes, and ſtrong enough to overſet negro huts and veſſels, or drive the latter from their anchors, and throw them on ſhore. The ſtorm abates, and heavy rain follows accompanied with much lightning and ſtrong claps of thunder. Sometimes tornados happen without rain, or at leaſt with very little, but then the ſtorm is more violent and laſts longer. It has been imagined by ſome, that this kind of ſtorm brings ſome peſtiferous quality with it, becauſe they had obſerved, that out of a number of people ſeveral fell ſick in one night after a tornado.

This I have in ſome degree experienced myſelf; for in the month of September 1776, feeling myſelf very well, and having dined as uſual, the ſtorm of a tornado ſuddenly tore down the window-ſhutters, and blew into the room where I was: about an hour after I had rigors,

and in the evening I had a high fever, which turned out to be a very ſevere bilious one; but notwithſtanding this, it has, in my opinion, no ſuch ill quality, and the above phenomenon may be attributed to the change it produces on the air, and of conſequence on the body; it may therefore be conſidered as the occaſional cauſe of a diſorder to which the body was pre-diſpoſed long before.

The dampneſs of the atmoſphere during this ſeaſon is ſo great that it is more or leſs perceptible in every thing. Leather, wearing apparel, and books, grow mouldy. Poliſhed metals grow ruſty. Sea ſalt, ſugar, and other ſaline ſubſtances, which were perfectly dry before, melt; and the meat of cattle killed in the evening is ſpoiled the next morning, ſo as not to be fit for uſe.

Calms are very frequent and diſagreeable on account of the muſquetoes and other inſects, which then quit their retreats from among the mangroves and marſhes, and ſpread over the face of the country.

The dry or healthy ſeaſon begins commonly about the middle of October and laſts to the middle of July. It is called dry, becauſe then it hardly ever rains, or at leaſt but very ſeldom; and healthy, in oppoſition to the ſickly one: for though pleuriſies and peripneumonies will happen in the months of December and January, and fluxes

fluxes in the months of April, May, and June, few people die, which, when compared with the numbers that die in the other ſeaſon, juſtifies the denomination. When the rains ceaſe, the wind ſhifts its quarter, and is for the moſt part Eaſt or North-eaſt in the morning; but as the Sun riſes on the horizon, the wind changes more and more towards the North, till about noon, ſooner or later, it gets to the Weſt of North, which is called ſea-breeze, and is very refreſhing, though it happens ſometimes, that as the Sun falls again on the horizon, the wind will ſhift again towards the Eaſt, and continue there all night. This wind blows ſometimes very ſtrong, and is always exceſſively hot, drying up the lakes and pools, which had been formed by the heavy rains and the overflowing of the river, and producing in ſuch as partake of ſea water, a fine ſea ſalt in large cryſtals, not unlike foſſil ſalt. In the months of February, March, April, May, and June, the wind blows almoſt conſtantly from between North and Weſt, called ſea-breeze, except now and then a day or two it will be Eaſt, which when it happens in April makes it exceſſively hot, the Sun being then in and about the zenith of Senegal, heating the vaſt plains of ſand over which this wind is to paſs before its arrival there, which, reverberating the received heat, may contribute to increaſe it; for I have obſerved, that the

ſame month in the river Gambia was not hotter than any other wind, owing in all appearance to the difference of the ſoil of the country, which is not ſandy like that of Senegal. I think it is the duſt of the ſand raiſed by this wind which makes the atmoſphere look hazy. I myſelf ſaw in the year 1775, in the month of April, in a morning preceded by an Eaſterly wind, ſuch a duſt imitating a fog in the air, that one could not ſee above twenty yards.

The weather grew calm, and about eleven o'clock in the forenoon the atmoſphere grew clear by depoſiting a browniſh impalpable duſt, which covered every thing near, a line in thickneſs. The ſame thing I obſerved at ſea from on board of a veſſel in the month of March 1775, at the diſtance of about five or ſix leagues from the land near the latitude of Senegal. The wind having blown Eaſt in the night, I found in the morning the ſails, ſhrouds, and deck, covered with an impalpable duſt. The deſcription given by the learned Dr. LIND [(a)] of the Harmattans of the Coaſt of Guinea, ſeems to agree with the Eaſt wind at Senegal in almoſt every reſpect, except that the damp vapour in the former is not perceptible in this, for it dries every thing that will admit of it. Water poured on the floor of a

(a) Eſſay on the moſt effectual Means of preſerving the Health of Seamen.

room

room for the purpoſe of cooling the air, is dried up in an inſtant, and there is ſome effect on the thermometer placed in ſuch a room. Salt, ſugar, and the like ſubſtances, which are half melted by the damp air during the rainy ſeaſon, dry again in a few days into hard lumps. Such houſhold furniture as is made of wood, though it has been ever ſo well ſeaſoned, ſhrinks and grows looſe where joined, or ſplits and cracks where glued. It dries and parches the ſkin of the white people as well as the blacks, and makes it ſometimes as rough as any clear froſty weather in Europe would. The ſky is commonly clear and without clouds; but the atmoſphere is hazy, which, in my opinion, as I have already obſerved, is occaſioned by the duſt, perhaps in conjunction with vapours ariſing from the ſurface of the earth and waters. Theſe vapours, though not to be ſeen in the open air, I have perceived by their ſhadow upon white walls, ariſing from pools which were cloſe to them; but the air being ſo dry they are abſorbed by it, and no more perceptible as vapour. That the evaporation muſt be very great when this wind blows, the method the blacks have of cooling water will evince. They fill tanned leather bags with it, and hang them up in the Sun; the water oozes more or leſs through the leather, ſo as to keep the outward ſurface of it wet, which, by its quick and con-

 tinued

tinued evaporation, occafions the water within the bag to grow confiderably cool.

This wind is in general not reckoned unwholefome, either by the inhabitants or Europeans, though it feels very difagreeable, and by depriving the body of its thinner fluids may be looked upon as the immediate caufe of fome difeafes, and the pre-difpofing one to others. When it fets in fooner or later in the month of October, it is confidered by the inhabitants as producing a ceffation of the fickly weather, and the beginning of healthier. In the months of December and January, when the Sun is at its greateft diftance, it makes the weather feel very cold in the nights and the mornings.

The putrid diforder, which proved fo fatal to the garrifon and the inhabitants of Senegal, made its appearance in the beginning of Auguft. The preceding month of July had been remarkably healthy; though the weather was very hot and fultry, there were only three foldiers in the hofpital for flight venereal diforders; but we learnt by fome black meffengers, who came from Goree, that there was a fever raging there, which had carried off numbers of the French garrifon and inhabitants of the ifland, and we thought ourfelves very happy not to partake of their fate. On the fecond of Auguft one of the foldiers, who was in the hofpital for a gonorrhea, being

cured, was difcharged. The fourth of Auguft he was again reported to me as very fick in the barracks. I went to fee and found him in a high fever with the worft fymptoms. I ordered him to be carried to the hofpital, where he died the third day, with all the fymptoms of the greateft putridity. The orderly man of the hofpital was feized on the fixth of Auguft with the fame difeafe, and died the ninth. One of the venereal patients, who remained ftill in the hofpital, was taken with the fame fever, and died a few days after. Some of the foldiers of the fort, having accefs to the hofpital to vifit their fick comrades, took the contagion, and fpread it through the whole garrifon. I am apt to believe, that the diforder was brought to Senegal by the black meffengers from Goree; for I underftood that one of them had died foon after his arrival in Senegal, and it may be, that the foldier who died firft of it got the infection from them; for it is probable, that being difcharged the hofpital on the fecond of Auguft, and having leave to take a walk on the ifland on the third, he had been in company with fome of thefe black meffengers, or in the huts where they reforted, for the fake of hearing fome news from Goree, where he was acquainted. It may perhaps be obferved, that the foldier taking the contagion on the third of Auguft, it could not make fo rapid a progrefs as

to

to manifeſt itſelf the next morning in the higheſt degree; but this I intend to ſupport by the following caſes. One of the ſurgeon's mates dreſſed a bliſter on the back of a ſoldier, ill of the diſorder, with a digeſtive ſoftened with oil of turpentine: having done, he came into the ſurgery, and looked quite pale, telling me, That the ſoldier's back had ſmelled ſo putrid and offenſive, that it had made him quite faint and ſick at the ſtomach. He took ſome tincture of bark and bitters, and went home, when a fever, with a train of the worſt ſymptoms, made its appearance in the evening, and he died the third day. Another gentleman, who was ſent for by the ſaid ſurgeon's mate in the morning of the ſecond day of his illneſs, and requeſted to draw up a will for him, arrived while I was preſent. He ſpoke with the patient for a few minutes, and then took me aſide, ſaying, That there was a certain ſmell about the room, which made him faint and ſick at the ſtomach, and that he ſhould be obliged to retire; he did, but in the evening was ſeized with the fever and all its bad ſymptoms, went through ſeveral of its ſtages, but recovered. A black boy, who had been waiting on the ſaid ſurgeon's mate during his illneſs, was taken with the ſame diſorder, and died of it in a few days. I could produce ſeveral other caſes to ſtrengthen what I have advanced concerning the quick

appearance

appearance of the diſorder itſelf after the contagion had taken place, but I think the three related ones ſufficient.

The ceſſation of this contagious diſeaſe may be dated from about the middle of September. Governor CLARKE, who died the 18th of this month, concluded the dreadful ſcene. He had avoided the communication with all ſick people, but did not heſitate in admitting my company. I was the only one who dined with him for ſeveral weeks; and as I was continually among the ſick in the hoſpital and on the iſland (of the former of which I gave him a return every morning) I might probably have conveyed the infection to him in my cloathing, though I was not affected myſelf. A few people died in the months of October, November, and December; ſome of relapſes of the ſame fever, and others of ſevere fluxes and abſceſſes in the liver, in which the diſorder had terminated. It is remarkable, that a fleet of merchant-men, under convoy of a ſloop of war, which left Senegal on the fourth of Auguſt, and ſailed for England, had, by what I could learn, been entirely free from this diſorder; neither did it reach as far as the river Gambia, for the garriſon at Fort James in that river enjoyed a pretty good ſtate of health during all this time, and loſt only two men, who died of fluxes.

The thermometer I have ufed is FAHRENHEIT'S made by WILLCOX and COYSGARNE. It has been compared, fince my arrival in England with one made by RAMSDEN, and found to be about three-fourths of a degree lower. It was placed in the fort at Senegal, in a room two ftories high with a blank cieling, and above that a bevil roof covered with flate: this room was not expofed to the Sun but at its rifing and fetting, it being fheltered from it by other buildings, when high on the horizon. I am forry that I had not begun to keep the journal of heat and weather two or three months fooner, for the fatisfaction of the curious in natural philofophy; but as it required a degree of leifure which I had not, being obliged to attend to my duty, and as I obferved nothing extraordinary either in the heat or weather, and imagined that journals of this kind, and of this country, might have been publifhed in Europe long before now, I neglected it, but was tempted to keep it when the fatal difeafe made its appearance.

It has been obferved by Dr. LIND[(b)], that in that country a great change in the weather has little or no effect on the barometer. I have remarked the fame at Fort James Gambia in the year 1776, for I found from the fourth of February to the laft of April, that the alte-

(b) Effay on Difeafes incidental to Europeans in hot Climates.

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ration the weather produced on the barometer was ſo little as hardly to be perceptible. The equality of the weather during this time (which is part of the dry ſeaſon in which the ſky is always clear and without clouds, though the different winds produce ſenſible changes in the atmoſphere) may perhaps account for it; but Governor CLARKE, who had a barometer placed in one of his rooms in the fort at Senegal, told me, That the greateſt changes in the weather during the rainy ſeaſon had ſo little effect on that inſtrument that it was hardly worth notice.

I beg you would do me the honour to preſent theſe remarks, with the annexed journal, to the Royal Society, if you find them to contain any thing worthy the notice of that illuſtrious body.

I have the honour to be, &c.

1778	Hour.	Therm.	Wind.	Weather.
Aug. 10	11	82	NNW	Clear.
	2	83	NNW	A tornado coming on, which lafted an hour.
	4	81	NNW	Lightning to the Southward.
	8	81	NNW	Lightning and thunder to the Southward.
11	6	80	N	Clear.
	9	81	N	Clear.
	4	83	W	Lghtning and thunder with rain.
	8	81	W	Clear with lightning.
12	6	76	NNE	Thund. lightn. and rain during the whole night.
	11	80	S	Clear.
	3	82	N	Clear.
	8	82	——	Calm and clear.
13	7	80	SE	Small rain.
	11	82	SSW	Cloudy with Sun-fhine; clofe and moift.
	4	83	W	Clear.
	9	81	W	
14	7	80	W	Cloudy with fmall rain.
	11	82	W	Cloudy.
	4	83	W	Cloudy.
	9	82	W	Cloudy.
15	7	80	NNW	Cloudy.
	5	83	W	Cloudy.
	9	81	W	Cloudy.
16	7	80	SW	Clear.
	11	82	NW	Clear.
	3	83	NW	Clear.
	9	82	N	Clear.
17	7	79	S	Cloudy.
	10	80	W	Cloudy.
18	7	80	S	Cloudy.
	12	82	NE	Thunder and rain.
	2	78	W	Thick and mifty.
	8	79	WSW	Hazy.
19	7	78	ESE	Clear.
	11	81	S	Clear.
	4	84	NW	Cloudy with Sunfhine.
	9	82	——	Calm and clear.
20	7	79	S	Cloudy.
	4	84	W	Cloudy and thunder.
	9	82	W	Cloudy and rain.

1778	Hour.	Therm.	Wind.	Weather.
Aug. 21	7	79	SSE	A great deal of rain.
	1	78	SW	Cloudy and hazy.
	4	78	SW	Cloudy and hazy.
	7	80	S	Cloudy and ſhowers of rain.
22	6	78	SW	A great deal of rain.
23	7	78	——	Calm and clear, a great deal of rain the night before.
	2	81	SW	Cloudy.
	9	80	NW	Clear, lightning to the Eaſtward.
24	7	79	N	Cloudy.
	12	82	NNW	Clear.
	2	82	——	Thunder, a tornado coming on from the Eaſt.
	9	81	——	Calm and cloudy, a tornado with rain three hours before.
25	7	78	——	Calm, a heavy tornado with rain laſt night.
	1	81	N	Cloudy.
	4	81½	N	Cloudy.
	10	81	N	Clear.
26	7	80	N	Clear.
	11	80	E	Clear, a tornado with rain two hours before.
	1	81	——	Calm and clear.
	11	82	N	Clear.
27	7	78	E	Miſty, a great deal of rain the night before.
	11	78	SE	Cloudy, now and then rain.
	3	80	SE	Cloudy.
28	7	78	SW	Miſty and drizzling rain.
	2	82	SW	Cloudy.
	6	81	SW	Cloudy.
	9	80	W	Cloudy.
29	7	80	W	Cloudy.
	1	81	NW	Cloudy, blowing hard.
30	7	80	NW	Cloudy, the air very thick.
	10	82	SE	Thick and hazy.
	2	83	WSW	Hazy.
	8	82	W	Hazy.
31	7	78	NE	Thick and rain; a tornado laſt night, with a deal of rain, thunder, and lightning; before the tornado came on it was exceſſive cloſe.
	3	83	E	Cloudy.
	9	83	——	Calm, cloudy, and very cloſe; lightning all round the horizon.
Sept. 1	7	80	W	Cloudy.
	3	83	WSW	Clear.
	9	82	W	Clear and fine.

1778	Hour.	Therm.	Winds.	Weather.
Sept. 2	7	81	SW	Close and cloudy.
	1	83	W	Hazy.
	5	84	NW	Thunder towards the South-east.
	9	84	NW	Lightning towards the South-east.
3	7	81	SE	Close and cloudy; last night calm and very close.
	11	84	E	Clear.
	5	85	SW	Cloudy.
	9	82	W	Cloudy.
4	7	80	W	Cloudy.
	11	80	W	Showers of rain with thunder.
	9	81	NW	Cloudy.
5	7	80	NW	Cloudy.
	12	83	NW	Cloudy.
	9	82	NW	Cloudy.
6	7	81	——	Calm and cloudy.
	2	84	W	Cloudy.
	4	84	SE	A tornado with rain.
	9	82	——	Calm and hazy.
7	7	81	SW	Cloudy.
	1	82	SW	Cloudy.
	5	84	——	Calm and close, a tornado coming on from the SE.
	9	80	SW	The tornado ceased, cloudy and damp.
8	6	78	S	Cloudy.
	12	83	S	Hazy and cloudy.
	9	82	NW	Hazy and cloudy.
9	7	82	SE	Cloudy and close.
	11	83	SW	Cloudy.
	6	84	W	Hazy and close.
	9	83	W	Cloudy.
10	7	82	SE	Cloudy.
	3	83	W	Cloudy.
	5	85	NW	Clear.
11	7	82	NW	Cloudy.
	5	83	N	Cloudy.
	9	82	N	Cloudy.
12	6	80	N	Cloudy.
	12	82	N	Cloudy.
	5	83	NW	Cloudy.
	9	82	NW	Cloudy.
13	6	80	N	Cloudy.
	3	83	N	Cloudy.
	5	83	N	Cloudy.
	9	81	NW	Cloudy.

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1778	Hour.	Therm.	Winds.	Weather.
Sept. 14	7	81	SE	Cloudy and thick, laſt night very cloſe.
	12	83	SW	Cloudy and hazy.
	2	83	SW	A tornado coming on from the Eaſt.
	9	79	——	Calm, from three o'clock till now heavy rain.
15	7	79	SE	
	11	82	S	Cloudy and rain now and then.
	5	82	W	Cloudy and rain now and then.
	9	82	W	Cloſe and cloudy, lightning towards the Eaſt.
16	6	81	E	Cloudy, the night very cloſe.
	10	76	N	Two hours before a tornado from the Eaſt, with a deal of wind and cold rain.
	2	82	S	Cloudy.
	9	81	——	Calm, cloudy, and cloſe.
17	7	81	NNW	Clear, laſt night exceſſive cloſe, and millions of muſquetos.
	12	83	NW	Clear.
	5	84	NW	Clear.
	9	82	NW	Clear.
18				
19	7	82	——	Calm, the night before very cloſe.
	10	84	——	Calm, a tornado coming on from the Eaſtward.
	1	81	E	Clear, two hours ago a tornado with but little rain.
	5	84	NW	Clear.
	8	84	——	Calm and clear, the air filled with muſquetos.
20	7	81	N	Clear, the night before quite calm.
	1	84	WNW	Clear.
	9	84	W	Lightning to the Eaſtward.
21	7	81	E	Cloudy, laſt night a tornado with a great deal of wind, but not much rain.
	2	84	E	Cloudy.
	5	85	E	Cloudy.
	9	84	E	
22	8	79	E	Cloudy.
	1	85	——	Calm and clear.
	5	85	W	A tornado from the Eaſt.
23	11	84	NW	Clear.
	3	84	NW	Clear.
	9	83	NW	Clear.
24	7	82	S	Cloudy.
	10	84	SW	Hazy.
	3	87	——	Calm and clear.
	9	85	NNW	Clear.

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1778	Hour.	Therm.	Winds.	Weather.
Sept. 25	7	82	E	Hazy and thick; at three o'clock this morning a heavy tornado, with a great deal of rain, thunder, and lightning.
	11	84	E	Hazy and thick.
	2	87	SE	Hazy.
	5	88	SE	Hazy.
	9	84	NW	Clear.
26	5	—	—	A tornado from the E. with a great deal of rain.
	8	79	E	Clear.
	4	84	—	Calm.
	8	84	—	Calm, plenty of musquetos.
27	9	83	N	Clear.
	2	85	N	Hazy.
	4	84	N	Hazy.
	8	84	N	Hazy.
28	6	83	—	Calm and hazy.
	1	85	SW	Hazy.
	5	86	—	Calm and hazy.
	8	84	W	Hazy.
29	7	82	W	Hazy.
	1	85	NW	Hazy.
	9	84	N	Clear.
30	7	82	N	Clear.
	1	84	NNW	Clear.
	5	85	NNW	Clear.
	9	84	NNW	Clear.
Oct. 1	8	81	N	Clear.
	1	84	N	Clear.
	9	83	N	Clear.
2	6	79	N	Clear.
	11	83	NNW	Clear.
	9	83	—	Calm and hazy.
3	8	80	E	Cloudy and hazy.
	12	80	E	Hazy.
	9	81	E	Hazy.
4	6	76	SE	Cloudy.
	1	81	SE	Cloudy, a few drops of rain now and then.
	9	81	—	Calm and cloudy.
5	6	79	—	Calm and clear.
	1	82	W	Calm and clear.
	9	82	W	Cloudy.

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1778	Hour.	Therm.	Winds.	Weather.
Oct. 6	6	79	WSW	Cloudy, an hour before a tornado with rain and thunder.
	11	77	SE	Cloudy, 3 hours before a tornado with cold rain.
	2	77	W	Cloudy.
	9	79	N	Cloudy.
7	6	76	N	Clear.
	1	80	N	Clear.
	5	81	N	Clear.
	9	81	N	Clear.
8	7	80	N	Clear.
	10	82	N	Clear.
	5	84	N	Clear.
	9	83	N	Clear.
9	7	81	N	Clear.
	11	85	N	Clear.
	12	87	NNE	Clear.
	9	83	N	Clear.
10	7	80	W	Cloudy.
	11	82	W	Cloudy.
	1	83	SW	Cloudy.
	9	82	N	Cloudy.
11	6	80	——	Calm and cloudy.
	11	82	N	Clear.
	2	84	N	Clear.
	9	83	N	Clear.
12	6	80	NNE	Cloudy.
	12	82	NNW	Cloudy.
	9	82	N	Clear.
13	6	79	N	Clear.
	2	82	N	Clear.
	9	82	N	Clear.
14	6	79	N	Clear.
	10	80	N	Clear.
	4	81	N	Clear.
15	6	77	N	Clear.
	2	81	N	Clear.
	9	80	NNW	Clear.
16	6	78	N	Hazy.
	12	79	N	Clear.
	5	82	W	Clear.
	9	81	W	Clear.

1778	Hour.	Therm.	Winds.	Weather.
Oct. 17	6	79	W	Hazy, a heavy dew last night.
	12	82	——	Calm and hazy.
	9	81	W	Hazy, a dew.
18	6	79	W	Clear.
	12	82	W	Clear.
	6	83	W	Clear.
	9	83	W	Clear.
19	6	78	NNW	Clear.
	2	82	WNW	Clear.
	9	80	NW	Clear.
20	7	78	N	Clear, a heavy dew last night.
	2	82	NNW	Clear.
	9	80	N	Clear.
21	6	78	N	Clear.
	2	82	NNW	Clear.
	9	80	NW	——
22	7	79	NW	——
	1	81	N	Heavy dew last night.
	8	80	N	Clear.
23	6	76	N	Clear.
	11	82	NE	——
	9	81	W	Dew.
24	6	78	NW	——
	2	86	NE	Dusty.
	3	88	E	Dusty.
	4	90	NE	Dusty.
	5	92	E	Dusty.
	8	87	E	——
25	7	81	N	Clear.
	11	86	E	Dusty.
	3	92	E	Dusty.
	4	92	E	Dusty.
	9	83	N	——
26	8	77	NE	——
	4	85	N	Clear.
	9	81	N	Clear.
27	6	79	N	Clear.
	2	87	N	Clear.
	9	84	N	Clear.
28	6	79	E	——

1778	Hour.	Therm.	Winds.	Weather.
Oct. 29	6	78	N	Clear.
	11	84	NE	Dusty.
	1	90	NE	Dusty.
	2	92	NE	Dusty.
	9	83	NNW	——
30	6	78	N	Clear.
	12	86	NE	Dusty.
	9	83	NW	——
31	8	81	W	Hazy.
	11	82	W	Hazy.
	4	84	NW	Hazy.
	9	85	——	Calm.
Nov. 1	6	80	NNE	——
	1	84	NNW	Cloudy.
	5	83	N	Thunder and rain.
	9	83	N	Cloudy.
2	7	80	N	Hazy.
	1	90	NE	Dusty.
	4	91	ENE	Dusty.
	9	87	NNW	——
3	7	80	NNE	Dusty.
	12	90	NNE	Dusty.
	4	92	NNE	Dusty.
	9	85	NNW	——
4	7	80	——	Calm.
	11	82	NNW	Clear.
	5	83	NNW	Clear.
	9	82	NNW	Clear.
5	7	78	NNW	Clear.
	11	85	N by E	Clear.
	1	80	NNW	Clear.
	9	81	NNW	——
6	6	77	NW	Clear.
	3	82	NNW	Clear.
	9	80	NW	Clear.
7	6	78	NNW	Clear.
	11	79	NNW	Clear.
	9	80	NW	Clear.
8	7	79	SW	Thick and cloudy, dew last night.
	11	84	W	Hazy.
	9	80	W	Hazy.

1778	Hour.	Therm.	Winds.	Weather.
Nov. 9	7	80	——	Calm.
	1	89	NE	Dusty.
	4	91	NE	Dusty.
	9	84	N	——
10	7	78	NNE	——
	11	85	NE	Dusty.
	4	91	NE	Dusty
	9	83	NNW	——
11	6	76	NE	——
	2	91	E	Dusty.
	5	89	N	——
	9	82	E	——
12	7	76	——	Calm. Note, that from this date forward the sky and atmosphere have been always clear, when the wind blew from between North and West; but when it blew from between North and East, the atmosphere was more or less hazy and the sky clear, except on those days where there are particular remarks.
	2	89	E	
	3	92	E	
	9	83	NNE	
13	7	78	E	
	2	90	E	
	9	84	NNE	
14	6	76	ENE	
	2	90	NNE	
	9	83	NNE	
15	7	75	NE	
	4	87	NNE	
	9	82	NNE	
16	6	75	NNE	
	1	86	N by E	
	3	90	NNE	
	9	81	N by E	
17	7	75	NNW	
	11	81	N by E	
	4	82	NNW	
	9	80	NNW	
18	6	75	NNW	
	1	79	NNW	
	2	82	NNW	
19	6	74	NNW	
	12	79	NNW	
	2	79	NW	
	9	76	NNW	
20	7	74	NNW	
	11	76	NNW	
	9	78	NW	

1778	Hour.	Therm.	Winds.	Weather.
Nov. 21	7	73	NNW	
	1	85	NNE	
	3	88	NNE	
	9	79	NNW	
22	6	73	NNE	
	1	84	E	
	2	84	NW	
	9	78	NNW	
23	6	73	NNE	
	1	84	E	
	2	84	NW	
	9	78	NNW	
24	6	70	E	
	4	81	NE	
	5	79	NNE	
	9	78	NNE	
25	6	69	ENE	
	12	74	E	
	5	76	E	
	9	74	NE	
26	6	63	E	
	4	75	E	
	9	72	E	
27	6	64	E	
	12	74	E	
	6	74	NNE	
	9	72	NNE	
28	6	64	E	
	1	75	E	
	9	74	E	
29	6	64	E	
	11	72	E	
	4	77	N by E	
	9	72	NNE	
30	6	64	E	
	1	75	E	
	9	73	NNW	
Dec. 1	6	64	E	
	11	72	E	
	2	80	E	
	9	73	N	

1778.	Hour.	Therm.	Winds	Weather.
Dec. 2	6	64	E	
	1	79	E	
	4	77	NNW	
	9	73	NNW	
3	6	64	E	
	11	76	E	
	1	79	E	
	9	75	NNW	
4	6	65	NE	
	1	78	E	
	9	74	NNW	
5	6	67	NE	
	2	77	E	
	5	76	NNW	
6	6	68	NE	
	1	74	NNW	
	4	73	NNW	
7	6	68	NNE	
8	7	70	NE	
	1	76	NNE	
	9	73	N	
9	7	70	NNE	
	12	72	NNE	
	2	76	NNE	
	9	72	NE	
10	6	66	E	
	11	76	E	
	1	80	NE	
11	7	68	E	
	1	79	E	
	2	80	NNW	
	9	74	NNW	
12	7	65	NE	
	11	70	E	
	4	80	E	
	9	73	N	
13	7	66	NNE	
	11	76	E	
	3	80	E	
	9	74	NNW	

1778	Hour.	Therm.	Winds.	Weather.
Dec. 14	7	67	E	
	11	76	E	
	3	83	E	
	9	76	N	
15	7	71	E	
	11	78	E	
	5	86	E	
	9	79	NNE	
16	7	71	E	
	1	81	E	
	2	84	E	
	9	75	NNW	
17	7	69	NE	
	12	75	NE	
	4	78	N	
	9	72	N	
18	7	64	NE	
	2	84	E	
	4	79	N	
	9	73	N	
19	6	65	NE	
	1	80	E	
	9	75	NE	
20	6	62	NE	
	2	80	E	
	9	75	NNE	
21	6	64	NE	
	1	73	E	
	2	75	E	
	9	71	NNW	
22	6	64	NE	
	9	71	NNW	
23	6	64	E	
	12	78	E	
	2	81	NNE	
	9	73	NNW	
24	7	66	E	
	2	78	E	
	5	77	NNE	
	9	72	N	

1778	Hour.	Therm.	Winds.	Weather.
Dec. 25	7	63	NE	
	1	73	E	
	2	74	E	
	9	70	N	
26	7	58	NE	
	11	68	E	
	3	72	NNW	
	9	67	N	
27	7	60	NE	
	2	74	N	
	9	68	N	
28	7	62	NE	
	12	73	E	
	2	72	NNW	
	9	70	NNW	
29	6	70	E	Foggy and thick, drizzling rain.
	7	70	N	Small rain.
	11	72	E	
	1	75	E	
	9	72	N	A few drops of rain.
30	6	67	N	Thick and foggy, with a few drops of rain.
	12	71	NNW	Thick and foggy.
	4	73	N	Foggy.
	9	70	N	
1779 Jan. 1	7	64	NE	
	1	76	E	
	4	80	NNW	
	9	72	NNW	
2	7	63	E	
	11	68	NNW	
	3	71	NNW	
	9	69	NNW	
3	6	64	NNW	
	11	70	NNE	
	3	70	NNW	
	9	69	NNW	
4	6	63	N	
	12	73	E	
	9	70	NNW	
5	6	60	N	
	9	69	NNW	

1779

1779	Hour.	Therm.	Winds.	Weather.
Jan. 6	6	63	N	
	11	70	NNW	
	4	72	NNW	
	9	70	NNW	
7	6	62	N	Foggy.
	2	71	NNW	
	9	70	NNW	
8	7	67	NE	Cloudy and thick, lightning and rain laſt night.
	4	70	NNW	
	9	69	NNW	
9	7	67	N	Foggy.
	4	70	N	
	9	70	N	
10	7	67	N	
11	7	69	N	
	1	70	NNW	
	4	71	NNW	
	9	69	NNW	
12	7	67	N	
	2	70	NNW	
	4	70	NNW	
	9	69	NNW	
13	7	68	N	Dew laſt night.
	2	70	NNW	
	4	71	NNW	
	9	69	NNW	
14	8	68	N	Dew laſt night.
	1	69	NNW	
	4	69	NNW	
	9	68	NW	
15	9	68	NNW	Dew laſt night.
	11	69	NNW	
	4	70	NNW	
	9	69	NNW	
16	7	67	N	Heavy dew laſt night.
	11	68	NNW	
	4	69	NNW	
	9	69	NNW	
17	7	67	N	
	1	68	NNW	
	4	69	NNW	
	9	68	NNW	

1779	Hour.	Therm.	Winds	Weather.
Jan. 18	7	66	NE	
	12	70	NE	
	4	73	NE	
	9	71	NE	
19	7	63	NE	
	11	68	NE	
	4	72	NE	
	9	69	N	
20	7	59	NE	
	11	70	NE	
	4	75	NE	
	9	69	N	
21	7	60	N by E	
	1	73	NE	
	4	75	NE	
	9	67	NE	
22	6	58	NE	
	12	70	NE	
	4	75	NE	
	9	67	NE	
23	6	58	NE	
	12	69	NE	
	2	71	NE	
	9	67	NE	
24	6	59	NNE	
25	6	55	N by E	
	12	67	NE	
	4	70	NE	
	9	65	NE	
26	6	56	NE	
28.	6	55	NE	
	11	66	NE	

XXIX. *Aſtronomical Obſervations relating to the Mountains of the Moon. By Mr.* Herſchel *of* Bath. *Communicated by Dr.* Watſon, *Jun. of* Bath, *F. R. S.*

Read May 11, 1780.

AT the time when the teleſcope was firſt invented this noble inſtrument was immediately applied to aſtronomical obſervations with the moſt ſurpriſing ſucceſs. Several very eminent perſons have given us an account of their diſcoveries; and, notwithſtanding the imperfect ſtate of teleſcopes in thoſe times, we ſtill owe a great deal of our knowledge of the heavenly bodies to the obſervations that were made by thoſe firſt teleſcopic obſervers, who made amends for the deficiencies of their inſtruments by their uncommon diligence and attention.

It may, perhaps, be eſteemed to be a mere matter of curioſity to ſearch after the height of the lunar mountains. I grant that there are more neceſſary and more uſeful objects of inquiry in the ſcience of aſtronomy; but when we conſider that the knowledge of the conſtruction of the Moon leads us inſenſibly to ſeveral con-

 ſequences,

ſequences, which might not appear at firſt; ſuch as the great probability, not to ſay almoſt abſolute certainty, of her being inhabited, we ſhall ſoon agree, that theſe reſearches are far from being trifling.

My reaſon for repeating obſervations that have been made by very good aſtronomers was not that I doubted either their veracity or diligence. The names of GALILEO, HEVELIUS, KIRCHER, and ſeveral more, will always deſerve to be mentioned with particular reſpect for the eminent ſervices they have rendered to aſtronomy; but as we know that their inſtruments were far from being arrived to that degree of perfection we have now obtained, I thought it by no means improper or uſeleſs to repeat their obſervations on the lunar mountains, and to extend them to other parts of the Moon's viſible hemiſphere, and thereby to eſtabliſh this theory on the firmeſt evidence of a ſurvey taken by a very excellent inſtrument.

The method uſed by HEVELIUS and others to find the height of a mountain in the Moon is this. Let a ray of light SLM (fig. 1.) proceeding from the Sun, paſs by the Moon at L, and touch the top of a mountain at M: then the ſpace between L and M will appear dark, and the top of the mountain will be ſeen to ſtand at ſome diſtance from the illuminated part of the Moon's diſk. With a

 good

good micrometer let the diftance LM be taken by obfervation[(a)]. Draw LC perpendicular to LM; draw alfo MC from the top of the lunar mountain to the center of the Moon: then in the triangle MLC, rectangled at L, we have given the fide LC, which is the Moon's radius, and the fide LM taken by obfervation. Therefore, by trigonometry, we can find the hypothenufe MC[(b)], from which, fubtracting the part *p*C or radius, there remains the perpendicular height of the mountain M*p*. I have followed the fame method, as being the leaft liable to error.

GALILEO takes the diftance of the top of a lunar mountain from the line that divides the illuminated part of the difk from that which is in the fhade to be equal to a 20th part of the Moon's diameter; but HEVELIUS affirms, that it is only the 26th part of the fame.

When we calculate from thence the height of fuch a mountain it will be found, in Englifh meafure, according to GALILEO, almoft $5\frac{1}{2}$ miles; and, according to HEVELIUS, fomething more than $3\frac{1}{4}$ miles, admitting the Moon's diameter to be 2180 miles.

(a) I do not recollect that HEVELIUS mentions in what manner he took the diftance LM; but I am apt to believe it was by a micrometer.

(b) $\sqrt{\overline{LC}^2+\overline{LM}^2}=MC$.

He

He says, in his Selenography, p. 266. "Vera distan-
"tia illustratarum cuspidum, à confinio luminis et um-
"bræ, præsertim tempore quadraturæ, invenitur, unâ
"vigesimâ sextâ parte, totius Lunæ dimetientis con-
"stare; quando nimirum sunt remotissimæ: quemad-
"modum hoc ex phasi trigesimâ secundâ, monteque
"Apennino; ex phasi trigesima prima, monteque Di-
"dyme; et trigesimâ phasi, monteque Tauro et Anti-
"tauro, manifestissimè demonstratur." Having after-
wards mentioned that GALILEO makes the distance LM to
be the 20th part of the Moon's diameter, HEVELIUS pro-
ceeds, "Quamobrem, cum distantiæ a nobis designatæ,
"paululum sint minores, idcirco et montes aliquantu-
"lum depressiores inveniuntur, quàm GALILÆUS æsti-
"mavit: neque non tamen illi terrenis nostris montibus,
"quoad altitudinem, non solum æquiparari possunt meri-
"tissimò; sed et multo certe sunt excelsiores, quàm
"nostri omnium maximi; prout confestim, ex adjecto
"diagrammate patebit." He gives us then his calcula-
tion according to German and Italian measure; and hav-
ing found, in the manner above mentioned, the hypo-
thenuse MC, he adds: "Semidiameter Lunæ erit 1976
"octav. part. Si igitur hæc à totâ hypothenusa aufere-
"tur, restabunt adhuc sex, hoc est sex octavæ unius mil-
"liaris, vel tres quartæ unius milliaris Germanici, sive
"tria

" tria milliaria Italica: quæ eſt vera, et genuina altitudo " iſtius montis." As a German mile in the time of HEVELIUS was a very uncertain meaſure, we may ſuppoſe that he meant geographical miles, 15 of which make a degree of latitude. The obſervations of HEVELIUS have always been held in great eſteem; and this is moſt probably the reaſon why later aſtronomers have not repeated them. M. DE LA LANDE, who is one of our moſt eminent modern aſtronomers, agrees to the ſentiments above cited.

In his *Abrégé d'Aſtronomie*, p. 435. he ſays, " Je terminerai ce qui concerne la ſelenographie, en diſant un " mot de la hauteur des montagnes de la lune, qui étoient " quelquefois éclairées, quoiqu' éloignées de la ligne de " lumière de la treizième partie du rayon de la Lune; de " la on peut conclure que ces montagnes ont de hauteur " la 338ieme partie du rayon Lunaire, ou une lieue de " France. He then gives us a particular calculation, and the reſult is: " Avec ces données on trouve la hauteur de " 2643 toiſes, c'eſt à dire, plus d'une lieue commune."

He alſo mentions the opinion of GALILEO, and adds: " Mais on doit préférer à cet égard les obſervations " d'HEVELIUS, qui ont été plus répétées, plus détaillées " et plus exactes."

Mr.

Mr. FERGUSON ſays (Aſtronomy explained, § 252.) "Some of her mountains, by comparing their height "with her diameter, are found to be three times higher "than the higheſt hills on our earth."

KEILL, in his Aſtronomical Lectures, has calculated the height of St. Katherine's hill, according to the obſervations of RICCIOLUS, and finds it nine miles.

Before I report my own obſervations, it will be neceſſary to explain by what method I have found the height of a lunar mountain from obſervations that were made when the Moon was not in her quadrature; for the method laid down by HEVELIUS will only do in that one particular caſe: in all other poſitions the projection of the hills muſt appear much ſhorter than it really is. Let SLM, or *slm* (fig. 2.) be a line drawn from the Sun to the mountain, touching the Moon at L or *l*, and the mountain at M or *m*. Then, to an obſerver at E or *e* the lines LM, *lm*, will not appear of the ſame length, though the mountains ſhould be of an equal height; for LM will be projected into *on*, and *lm* into ON. But theſe are the quantities that are taken by the micrometer when we obſerve a mountain to project from the line of illumination. From the obſerved quantity *on*, when the Moon is not in her quadrature, to find LM we have the following analogy. The triangles *o*OL, *r*ML, are ſimilar; therefore,

therefore, $Lo : LO :: Lr : LM$, or $\frac{LO \times on}{Lo} = LM$; but LO is the radius of the Moon, and Lr, or *on*, is the obſerved diſtance of the mountain's projection; and Lo is the ſine of the angle $ROL = oLS$, which we may take to be the diſtance of the Sun from the Moon without any material error, and which therefore we may find at any given time from an ephemeris.

I will now give an account of my own obſervations relating to the mountains in the Moon; but, perhaps, it may not be amiſs to mention the inſtrument they were made with, and a few of the circumſtances, that it may appear how far their accuracy may be depended upon.

The teleſcope I uſed was a Newtonian reflector of ſix feet eight inches focal length, to which a micrometer was adapted conſiſting of two parallel hairs, one of which was moveable by means of a fine ſcrew. The value of the parts ſhewn by the index was determined by a trigonometrical obſervation of a known object at a known diſtance, and was verified by ſeveral trials. The power I always uſed, except when another is mentioned, was 222 times, alſo determined by experiment, which I have often found to differ ſomewhat from theory, on account of ſome little errors in the *data*, hardly to be avoided. The moon having ſufficient light, I uſed no more

more aperture of the object speculum than four inches; and, I believe, that for distinctness of vision this instrument is perhaps equal to any that was ever made.

OBSERVATIONS.

November 30, 1779, six o'clock in the morning, a rock, situated near what HEVELIUS calls *Lacus niger major*, was measured to project 41″,56. To reduce this quantity into miles, put R for the semi-diameter of the Moon in seconds, as given by the Nautical Almanac at the time of observation, and Q for the observed quantity, also in seconds and centesimals; then it will be in general $R : 1090 :: Q : \frac{1090\,Q}{R} = on$, in miles. Thus it is found that 41″,56 is 46,79 miles. This distance of the Sun and Moon at the same time was, by the Nautical Almanac, about 93° 57′$\frac{1}{2}$. The sine of which to the radius 1 is .9985, &c. and $\frac{on}{Lo}$ in this case, is LM = 46,85 miles. Then, by HEVELIUS's method the perpendicular height of the rock is found to be about one mile.

The same morning, a great many rocks, situated about the middle of the disk, projected from 25″,93 to 26″56. This gives *on* about 29,3 miles, and these rocks are all less than half a mile high.

January

January 13, 1780, 7 o'clock, I examined the mountains in the Moon; but there was not one of them that was fairly placed on level ground, which is a condition very neceſſary for an exact meaſurement of the projection. If there ſhould be a declivity on the Moon before the mountains, or a tract of hills placed ſo as to caſt a ſhadow on that part before them which would otherwiſe be illuminated, it is plain that the projection would appear too large; and, on the contrary, ſhould there be a riſing ground before them, it would appear too little.

As far as I was able to judge of the direction of the line of illumination, the higheſt hill projected 25″31, or 30,36 miles: from thence we find, as before that the perpendicular height is (.42 mile) leſs than half a mile.

January 14, 11 o'clock, I took the projection of the higheſt mountain which was ſituated at the Weſtern edge. It meaſured 24″, 68, or about 27 miles; and the perpendicular height comes out leſs than half a mile. There was not one mountain in the edge of the diſk ſo high as this.

January 17, 7 o'clock, a very high mountain projected no leſs than 40″,625. Its ſituation is in the South-eaſt quadrant. The Moon's ſemi-diameter, at the time of

 obſervation,

observation, by the Nautical Almanac, was 16′ 2″,6; therefore, $\frac{1090 Q}{R}$ = 45,98 miles = *on*.

	s	°	′	″
Sun's longitude at 7h. . .	9	27	39	0
Moon's longitude at 7 . .	2	2	46	52
Their nearest distance, . .	4	5	7	52

or about 125° 8′; the sine of which is .8104: thence we find LM 56,73 miles; and the perpendicular height of the mountain is 1m. 47, or less than a mile and a half.

January 22, 8h. 20′. the highest mountain, situated near Snell or Petavius, projected 11″,437, which is 12′,34; and LM comes out to be 35,3 mile: threfore the perpendicular height is ,57 mile.

Another, just behind Mare Crisium, measured only 7″, therefore is less than half a mile high.

January 25, 7h. 30′. in the morning, a mountain near Aristoteles measured 18″,59 which gives 20,6 miles; and LM is found 28,53 miles; the perpendicular height is therefore only ,37 mile.

Other mountains about Mare Nectaris measured about 23″,5; but they had hills before them, and their situation was not so proper for my purpose. However, it is evident they were of no considerable height.

January 28, 6 o'clock in the morning, the higheſt mountain in the diſk meaſured 30″,937; the Moon's ſemi-diameter at that time 15′ 40″; and *on* therefore equal 31,37 miles: but as the Moon is within four hours of her quadrature we may be aſſured that this mountain is leſs than half a mile high.

February 19, Mons Sinopium projected 5″,781; therefore *on* = 6,26 miles, and the quantity LM 56,54 miles; and conſequently the height of this mountain, which it ſeems proves to be a very high one, is not much leſs than a mile and a half. However, my journal obſerves, that the meaſure was very full; therefore the mountain in all probability does not exceed a mile and a quarter. Moreover, I think that obſervations made ſo near the full or new Moon are leſs to be depended upon, becauſe a ſmall error in meaſuring will produce a great one in the height of a mountain.

From theſe obſervations I believe it is evident, that the height of the lunar mountains in general is greatly over-rated; and that, when we have excepted a few, the generality do not exceed half a mile in their perpendicular elevation. It is not ſo eaſy to find any certain mountain exactly in the ſame ſituation it has been meaſured in before; therefore ſome little difference muſt be expected in theſe meaſures. Hitherto I have not had an

opportunity of particularly obferving the three mountains mentioned by HEVELIUS; nor that which RICCIOLUS found to project a fixteenth part of the Moon's diameter. If KEILL had calculated the height of this laft mentioned hill according to the theorem I have given, he would have found (fuppofing the obfervation to have been made, as he fays, on the fourth day after new Moon) that its perpendicular could not well be lefs than between eleven and twelve miles.

I fhall not fail to take the firft opportunity of obferving thefe four, and every other mountain of any eminence; and if other perfons, who are furnifhed with good telefcopes and micrometers, would take the quantity of the projection of the lunar mountains, I make no doubt, but that we fhould be nearly as well acquainted with their heights as we are with the elevation of our own. One caution I would beg leave to mention to thofe who may ufe the excellent $3\frac{1}{2}$ feet refractors of Mr. DOLLOND. The admirable quantity of light, which on moft occafions is fo defirable, will probably give the meafure of the projection fomewhat larger than the true, if not guarded againft by proper limitations placed before the object glafs. I have taken no notice of any allowance to be made for the refraction a ray of light muft fuffer in paffing through the atmofphere of the Moon,

when

when it illuminates the top of the mountain, whereby its apparent height will be leſſened, as we are too little acquainted with that atmoſphere to take it into conſideration. It is alſo to be obſerved, that this would equally affect the concluſions of HEVELIUS, and therefore the difference in our inferences would ſtill remain the ſame.

Bath, February 28, 1780.

Continuation of the ſame obſervations.

March 11, 1780, 7h. Promontorium Archeruſia projected 17",187. It is very properly ſituated for meaſuring. By a proper deduction from the Moon's ſemidiameter, as given by the Nautical Almanac, at the time of obſervation, we find the quantity *on* = 20.1 miles, and LM 22,6 miles; from which it appears, that the perpendicular height of this mountain is a little leſs than a quarter of a mile.

Antitaurus, the mountain meaſured by HEVELIUS was badly ſituated, becauſe Mount Moſchus and its neighbouring hills caſt a deep ſhadow, which may be miſtaken for the natural convexity of the Moon. A good, full, but juſt meaſure, 25",105; in miles 29,27: therefore,

LM

LM 31,7 miles, and the perpendicular height not quite half a mile.

7h. 45′. I was deſirous of being very exact in this meaſure, therefore I repeated it. I took two different obſervations. A narrow meaſure 21″,562; quite full enough 24″,062. Theſe meaſures give the perpendicular height leſs than half a mile.

8h. I meaſured Lipulus, 19″063. It is alſo badly ſituated, though rather better than Antitaurus. I found that the projection increaſed, therefore concluded that this was not the higheſt part of the mountain, and waited ſome time when I meaſured it again.

9h. Lipulus now projected 28″,75.

10h. It meaſured 28″,75: this gives *on* = 33,64 miles. Diſtance of Sun and Moon about 63° 23′: therefore, LM 37,54 miles. From hence we find the perpendicular height ,64 mile, or very near two-thirds of a mile.

March 12, 1780, 7h. One of the Apennine mountains, between Lacus Traſimenus and Pontus Euxinus projected 44″,062. This gives us *on* = 51,11 miles; and LM = 52,9 miles: therefore the perpendicular height of theſe mountains, which I know to be very high, comes out to be 1 $\frac{1}{4}$ mile.

Mons

Mons Armenia (near Taurus) projected 31″406 = 36,43 miles; LM = 38 miles nearly, and the height two-thirds of a mile.

Mons Leucopetra 34″479 or 40 miles; LM = 41,4 miles, and the perpendicular height three quarters of a mile.

There was a very fine ſhade of a high rock near it, which ſhewed the direction of the illuminating ray, and thereby aſſiſted me in meaſuring to a great exactneſs; but the mountain itſelf is not very favourably ſituated.

March 16, 10h. 30′. Mons Lacer projected 45″,625; but I am almoſt certain that there are two very conſiderable cavities or places where the ground deſcends below the level of the convexity, juſt before theſe mountains, ſo that theſe meaſures muſt of courſe be a good deal too large: but ſuppoſing them to be juſt, it follows, that *on* is 50,193 miles, LM = 64 miles, and the perpendicular height above $1\frac{3}{4}$ miles.

Another of the ſame mountains ſituated on the borders of S. Sirbonis meaſured 41″,875. This ridge of mountains is the ſame of which I meaſured one on January the 17th, which was then found to be 1,47 miles high.

The

The following additional Memoranda of the Manner in which Mr. HERSCHEL *made his Observations are taken from a Letter of his to the rev. Dr.* MASKELYNE, *Astronomer Royal.*

IN the second figure of my observations, the points L, S, E, *r*, are all supposed to be in one plane; and as the illuminating ray SL is also in this plane, it follows, that the line L*n* (= *on*) will always be perpendicular to the right line which joins the cusps of the Moon (c); and the truth of the theorem there delivered depends upon this circumstance.

For this reason I have taken care in all my observations to measure the line, which in fig. 3. (taken from your letter to Dr. WATSON) is marked *on*, parallel to the line CD, or perpendicular to AB, and not the line *rn*, perpendicular to the elliptical curve A*ro*B.

The manner of taking it is easy enough: however, I have occasionally used three different methods, and will describe

(c) It is here supposed, that rays from the Sun S, and the eye of the observer E, to any part of the Moon L, may be taken for parallel; and therefore, that different planes, made by several sections of the Moon, according as the point L is taken North or South of the diameter of the Moon, which is at rectangles to the line joining the cusps, may also be taken to be parallel to that diameter.

ſcribe them all, which I ſhould have done in the paper delivered by Dr. WATSON, had I not feared to be too particular.

The firſt method I uſed was to ſet the immoveable hair *bb* (fig. 4.) of my micrometer parallel to a line AB, joining the cuſps of the Moon; then, by opening the moveable parallel hair till it included the projection *on*, intended to be taken, I marked that down as the meaſure of *on*. As this method required ſome attention (that part of the ellipſis of illumination AVB which is the vertex V of the leſſer axis may ſerve as a direction) and took up ſome time, on account of the ſmall field of view of my teleſcope, I uſed occaſionally theſe two following ways.

When there was any remarkable figure on the diſk of the Moon near the line of illumination, I put on a compound eye-piece whoſe magnified field of view is full 40°, and power about 90 times, ſo that it takes in the greateſt part of the whole Moon; by this means I was enabled to view the projection intended for meaſuring at the ſame time with the reſt of the Moon, and to fix upon ſome mark in the diſk very near to its edge towards which I judged the line *on* ſhould be directed; then putting on the eye-piece which carries the micrometer I

took the distance according to this judgment as well as I could.

The third method I took was the following, which, indeed, I look upon as the best of all, and which I therefore most frequently put in practice. I took a view of some neighbouring shades of rocks or mountains, if there happened to be any near, and directed the measure of the micrometer by them, as they plainly pointed out the direction of the illuminating ray; or, which is the same thing, indicated the line perpendicular to a line joining the cusps.

Mons Leucopetra was measured by this last method, which circumstance I have mentioned in my observations of the 12th of March, where I saw the whole rock and its highest point, as well as the whole shade, and its last termination, upon very even ground, at the same time that I directed my micrometer in that line, to take the projection *on*, of the above mentioned mountain.

Sometimes I compared together a measure taken in the direction *on*, and one taken in the direction *rn*; but as most of my observations were made upon mountains not situated near the cusps or limb of the Moon, I never found so much difference between these two measures, that it could have occasioned any very material error, if I had intirely neglected it.

By the nature of the ellipsis it will appear, that, when we do not come too near the limb or cusps of the Moon, a tangent drawn to a point in the curve of illumination will seldom make with the subtangent an angle that exceeds (or is so much as) 26°; and in all such cases the error that can arise from taking the line *rn* instead of *on* will be less than the tenth part of the whole measure: but, if the angle the tangent makes with the subtangent is only about 18°, the error will be less than a 20th part; and all the measures I have taken, I believe, will be found to be much within these last-mentioned limits. From this consideration it will appear, that if I had not been aware of this circumstance, my observations would still be sufficiently accurate to disprove the usually assigned great height of the lunar mountains; but as I took all the precaution the situation of each mountain would afford, by using any one of the above mentioned three methods, which suited best, I believe there can hardly be a possibility of any error that should amount to a 40th part of the whole height of any mountain I have measured.

The figure ABCD (fig. 5.) contained by the diameter AB, the arch CD, and the two curve AD, BC, shews in what portion of the Moon's semi-disk we may safely measure the line *rn*, instead of *on*, without being liable

 to

to ſo great an error as one tenth part of the whole, and the figure AB*cd* contains that part wherein the meaſure *rn* being taken inſtead of *on*, the error will be leſs than the 20th part of the whole meaſure. In a portion ſomething more confined the error will ſoon vaniſh, ſo that the difference may be ſafely neglected intirely. Thus in the ſpace AB*xy* the error cannot amount to a hundredth part. Theſe figures may be conſtructed by taking the ſeveral points D, *d*, *y*, and C, *c*, *x*, 26°, 18°, 8°, reſpectively, from the vertex, the curves AD, A*d*, A*y*, BC, B*c*, B*x*, being the loci of thoſe points of the tangents which touch the ſeveral ellipſes of illumination that may be contained in the ſemi-diſk of the Moon, when theſe tangents make thoſe ſeveral angles of 26°, 18°, 8°, with their ſubtangents.

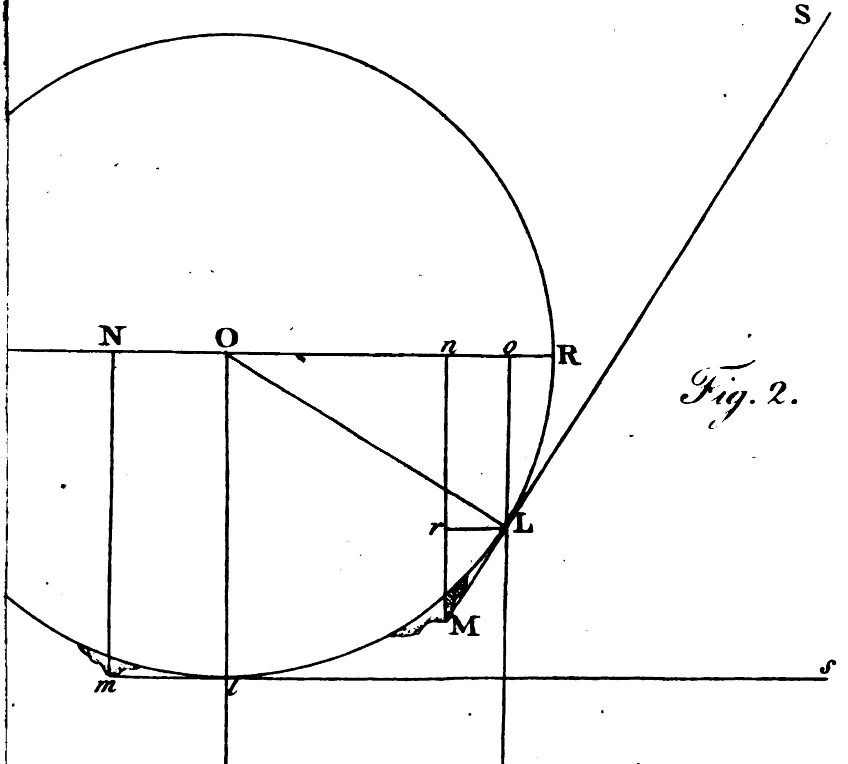

Fig. 2.

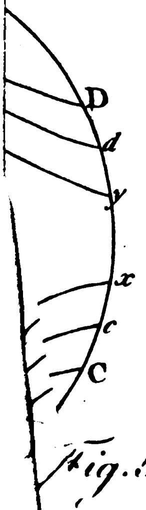

Fig. 5.

XXX. *Account of an extraordinary Pheasant.* By *Mr.* John Hunter, *F. R. S.*

Read June 1, 1780.

EVERY deviation from that original form and structure which gives the distinguishing character to the productions of nature may not improperly be called monstrous. According to this acceptation of the term, the variety of monsters will be found to be infinite. As far as my knowledge has extended, there is not a single species of animal, nay there is not a single part of an animal body, which is not subject to extraordinary formation. Nor does this appear to be a matter of mere chance; for it may be observed, that each animal has a disposition to deviate from nature in a manner peculiar to itself. It is likewise worthy of remark, that each species of animal is disposed to have the same kinds of supernumerary parts, and nearly the same kinds of defects; but every part is not, perhaps, subject to a great variety of forms, each part of each species having its monstrous

form, as it were, originally impressed on it by the hand of nature.

It is well known, that there are many orders of animals which have the two parts, designed for the purpose of generation, different in the same species, and which are thus divided into male and female: but this is not the only distinguishing mark in many genera of animals; in the greatest part of animals, the male being distinguished from the female by various marks.

A lion is different from a lioness; a cock from a hen, &c.; particularly the voice in many animals of the same genus is different: such I shall call secondary properties, which take place only in parts that are neither essential to life, nor generation, and which do not take place till towards the age of maturity. However, this distinction of sexes, by any other marks than the difference of the parts of generation, is not observable in every order of animals; for instance, there is very little difference between a dog and a bitch, exclusive of the parts of generation.

In those orders of animals, which are composed of distinct sexes, we may observe, the genital organs not only subject to a mal-conformation, similar to a mal-conformation in any other part of the animal; but we may likewise sometimes observe an attempt to unite the

two

two parts in one animal body, producing an animal called an unnatural hermaphrodite[a].

It is my intention at present to extend my inquiry on this subject no farther than as to what relates to that resemblance which one sex bears to that of another in those distinguishing properties which I term secondary.

The common class of unnatural hermaphrodites appear to be governed by certain laws, by which such an extraordinary formation of parts is effected; for it is observable, that these deviations obtain through whole species of animals precisely in the same manner. I have given an account of the Free-martin in a paper already presented to this Society. This hermaphrodite exhibits a mixture of the two parts of generation in the same animal.

But we find, however, that there is often a change of the secondary properties of one sex into another, the female now and then assuming the peculiarities of the male with respect to the scondary properties; and I may observe, that some classes are more subject to this than others; a singular example of which is to be the subject of the following paper.

Here I beg leave to premise, that in all animals of no distinct sex, there is no alteration taking place in their

(a) Vide Phil. Transf. vol. LXIX. part I.

form

form when at the age of maturity, which I have obferved not to be the cafe with moft of thofe animals which are of diftinct fexes, for in fuch the fecondary marks of diftinction are exhibited at certain periods of life.

There is no fex of any animal whatever that has any peculiarity in fhape when born, or when young; but moft of thofe animals, which are of diftinct fexes, have peculiarities towards the age of maturity. The male at this time lofes that refemblance which he before bore to the female in various fecondary properties, exclufive of what relates to the organs of generation (b). That it is the male who at this time recedes from the female in this refpect is evident. Every female, juft at the age of maturity, is more like the young of the fame fpecies than the male is obferved to be; and if the male is deprived of his teftes when young, he retains more of the original form, and therefore is more fimilar to the female.

From hence it might be fuppofed, that the female character contains more truly the fpecific properties of the animal than the male; but the true character of every animal is that which is in both fexes, *viz.* a natural hermaphrodite, or an animal of neither fex, *viz.* a

(b) This is not common to all animals of diftinct fexes; for in the fifh there is no great difference, nor in many infects, nor in dogs, as has been already obferved; it appears to be moft fo in birds.

castrated

caſtrated male or ſpayed female. Of the firſt we may inſtance the ſnail, which is of both ſexes, and has but one character, but that of the joint character of both ſexes.

But where the ſexes are ſeparate, and in ſpecies which have two characters, neither of them can be called the true one; the true diſtinguiſhing properties being thoſe peculiar to neither ſex, which are found in the caſtrated male, the ſpayed female, or the monſtrous hermaphrodite. That this is the diſtinct character of ſuch animals is evident, for the caſtrated male and the ſpayed female have but one ſet of properties between them; and when I treated of the Free-martin, which is a monſtrous hermaphrodite, I obſerved, that it was more like the ox than the cow or bull, ſo that the double ſex which contains the true character of every animal is imitated when made of no ſex by art, and by that means gives us the true properties of the ſpecies.

In the Free-martin the character ariſes from a mixture of ſexes; but in ſome animals, which have ſecondary principles peculiar to the two ſexes, we have a deviation from all thoſe general rules. We have in ſome a change of thoſe ſecondary characters, the perfect female with reſpect to the parts of generation, aſſuming more or leſs of the ſecondary character of the male.

 This,

This, however, does not appear to be a principle the action of which takes place at the first formation of the animal, so as to grow up with it, but appears to be one of those actions which take place, perhaps, at certain periods of life, similar to many common and natural phenomena; like to what is observed of the horns of the stag, which differ at different ages; or to the mane of the lion, which does not grow till after his fifth year, &c.

This change has been observed in some of the bird tribe, but principally in the common pheasant.

It is remarked by those who are conversant with this bird, when wild, that there appears every now and then a hen pheasant with the feathers of the cock; and all that they have decided on this subject is, that this animal does not breed, and that its spurs do not grow. Some years ago one of these was sent to Dr. HUNTER, who gave me leave to examine it. I found, upon examination, that it had all the parts of the female peculiar to that bird. This specimen is still preserved in Dr. HUNTER's Museum.

Dr. PITCAIRN, having lately received a pheasant of this kind from Sir THOMAS HARRIS, exhibited it as a curiosity to Mr. BANKS and Dr. SOLANDER. I happened to be then present, and was desired to examine the bird. The following is the result of my examination.

I found

I found the parts of generation to be truly female: they were as perfect as in any hen pheasant that is not in the least prepared for laying eggs. There were both the ovaria and the ovi-duct.

As these observations have hitherto been principally made upon birds that are wild, little more can be known of them; but from what happened to a hen pheasant belonging to a lady well known to the President, it should seem probable, that this character originates from a change at a late period of the animal's life, and does not originally grow up with it. This lady for some time had bred pheasants, and had paid particular attention to them. One of her hens, after having produced several broods, moulted, and the succeeding feathers were those of a cock. This animal was never afterwarde impregnated. Hence it is most probable, that all those hen pheasants which are found wild, and have the feathers of the cock, were formerly perfect hens, but that they are now changed by age, and perhaps by certain constitutional circumstances.

This change of feather in hen pheasants, although perhaps more common in them than in any other bird, yet is not absolutely peculiar to them; for we have a well attested instance of the same phenomenon in a pea-

hen, nearly under the ſame circumſtances as have been above deſcribed.

Lady TYNTE had a favourite pyed pea-hen, which had produced chickens eight ſeveral times; having moulted when ſhe was about eleven years old, ſhe aſtoniſhed the lady and her family by the feathers peculiar to the other ſex, and appearing like a pyed peacock. In this proceſs the tail, which was ſimilar to that of a cock, firſt appeared after moulting. In the following year, ſhe moulted again, and produced the ſame feathers. In the third year ſhe did the ſame: at the ſame time ſhe had ſpurs ſimilar to thoſe of a cock. She died in the following winter during the hard froſt, namely, in the winter $177\frac{5}{6}$. She never bred after this change in her plumage. This bird is now preſerved in the Muſeum of Sir ASHTON LEVER (b).

From what has been related of theſe two birds, may it not reaſonably be inferred, that it ſeems probable, that all thoſe wild pheaſants of the female ſex, which are

(b) It might be ſuppoſed, that this bird was really a cock which had been changed for a hen; but the following facts put this matter beyond a doubt. Firſt, there was no other pyed pea-fowl in the country. Secondly, the hen had knobs on her toes, which were the ſame after her change. Thirdly, ſhe was as ſmall after the change as before, therefore too ſmall for a cock. Fourthly, ſhe was a favourite bird, and was generally fed by the lady, and uſed to come for her meat, which ſhe ſtill continued to do after the change in the feathers.

found

found with the feathers of the cock, had changed the nature of their feathers, particularly at a certain age?

If this idea be juſt, it ſhews, that there is a diſpoſition in the female to come nearer and nearer to the male, at leaſt in the ſecondary properties; or it may rather be ſaid, that the female is later in producing this change than the male is; for it has already been obſerved, that both ſexes when young differ not from each other in theſe reſpects, but that the male appears to be the one that by degrees ſeparates itſelf from the female in its ſecondary properties.

XXXI. *A Letter to* Joseph Banks, *Esq. President of the* Royal Society, *&c. from* Daniel-Peter Layard, *M. D. Fellow of the Royal Societies of* London, Antiquaries, *and* Gottingen, *&c. relative to the Distemper among the horned Cattle.*

Read June 15, 1780.

S I R,

YOUR noble predeceffor, the late Earl of MACCLESFIELD, thought that every information relative to the afcertaining the nature of the diftemper among the horned cattle, and to confirm the utility of inoculation, was worthy attention, and the notice of the Royal Society.

His lordfhip was pleafed, in 1757, to lay before the Society my letter, which is inferted in vol. L. of the Philofophical Tranfactions; and now, SIR, to a worthy and refpectable fucceffor, whofe life, health, and fortune, have been moft generoufly adventured for the promoting natural knowledge, I addrefs this, to impart the refult of a long and ftrict inquiry.

In consequence of the essay which I published in 1756, I was called upon in 1769, by government, to assist with my advice towards the stopping the progress of the contagious distemper among the cattle, which had broken out in Hampshire: and by mere accident I discovered how the infection was brought from Holland to London, and was conveyed into that county.

Speedily and effectually to extirpate the calamity, no assistance was permitted to visit the infected villages, lest the farmers should be induced to prolong the illness, by attempting to cure their cattle; but positive orders were issued that all the cattle should be killed and buried properly, by which vigorous and salutary directions the distemper ceased intirely in a short time.

The same acts of parliament and orders of council, to kill the cattle and bury them deep, succeeded also soon after in North Britain; and to the former acts and orders issued in his late Majesty King GEORGE the second's reign, these alterations were made: to order that the infected cattle should be killed, without effusion of blood, by strangling; the hides to be neither cut or slashed; but the carcasses buried whole; and that all the fodder, litter, excrement, &c. should be buried, instead of being burned.

Since

Since that time the contagious diftemper has been brought twice into Effex, and once into Suffolk, from Holland, and as often ftopped by the fame means.

His Majefty having moft gracioufly been pleafed in April 1770 to appoint me to hold a foreign correfpondence, the orders and regulations which had happily fucceeded in Great Britain were communicated to the Dutch, the Flemifh, and the French, and copies of all papers delivered to Baron NOLCKEN, the Swedifh minifter. In Flanders, and Picardy in France, the fyftem of killing was adopted, and fucceeded. Afterwards in 1774, when the fame contagion was carried into the South of France from Holland through Bourdeaux, many attempts having failed to effect a cure, the devaftation was at laft ftopped by no other means than by killing the cattle, as in Great Britain. And here I beg leave to obferve, that Monf. VICQ. D'AZYR, in his *Expofé des Moyens Curatifs et Préfervatifs contre les Maladies peftilentielles, des Betes à Corne*, publifhed by authority at Paris in 1776, fays, p. 577, "That the falutary effects "of the precautions taken in the Auftrian Low Coun- "tries had excited the attention of the Englifh, who by the "fame means got rid of the fame calamity. They have "exactly and fcrupuloufly tranflated and put into exe- "cution the edicts iffued from the Juntos of Ghent and

"Bruſſels, and their undertaking has been crowned with "the moſt complete ſucceſs."* Monſ. VICQ. D'AZYR was miſinformed; for, on the contrary, the late Mr. Conſul IRVINE tranſmitted the acts of parliament, the orders of council, and my papers, containing every neceſſary information, to a member of the Junto of Ghent, whence they were ſent to the government at Bruſſels; and it was a long while before the Juntos could be prevailed upon to adopt the ſyſtem of killing, as they called it. It originated in England in 1747; and it is certain, that the Court of Vienna knew fully the obligations which the Auſtrian Netherlands had to the Britiſh government; whoſe orders and regulations had been implicitly followed, and which Monſ. VICQ. D'AZYR, ſays, p. 585, "He had modified and adapted to the rules of French "government."

In Flanders the infection was alſo prevented from ſpreading a ſecond time by the ſame method of proceeding; but unfortunately in Holland the cattle continue to be expoſed to the ſame diſeaſe. The half-yearly returns which have been regularly ſent me contain melancholy accounts of the ſevere loſs of cattle; ſometimes the whole have periſhed; at other times two-thirds have died; and generally above half fell when the ſickneſs was leſs violent. In a country where the illneſs is become

general, and constantly raging more or less, where the system of killing the cattle cannot now be thought of, and where inoculation has met with so many opponents of all ranks, there can be no other hope of getting rid of the calamity than by admitting into the United Provinces no other cattle than such which are found, or recovered from the infection [(a)].

I shall not trouble you, SIR, with the returns from Holland, or the tables of inoculation in Denmark, which would too much increase the length of this letter, but only mention, that in Denmark, where the contagious distemper is become naturalized and general, the Danish government have not only wisely adopted the orders and regulations issued in Great Britain, but have with unwearied application pursued the practice of inoculation. Count BERNSDORFF and Dr. STRUENSEE had all the necessary instructions, books, and papers, delivered to them by me, when the King of Denmark was in England; and I am assured by DANIEL DELAVAL, Esq. lately his Majesty's envoy-extraordinary at that Court, that inoculation is approved, recommended, and by authority established. Even in the first three years that inoculation was practised, of near three hundred head of cattle which were inoculated in a Danish island, not a sixth

(a) By the last half-yearly return from Holland, the number of infected cattle was so small, that it was hoped no further return to the States would be necessary.

part

part were loſt, notwithſtanding the many diſadvantages which unavoidably occurred.

Profeſſor CAMPER had before attempted to introduce inoculation in Holland; but the learned Profeſſor's abilities, diligence, and perſeverance, were ſo much counteracted by the obſtinacy and interruption of the peaſants, the badneſs of the ſituation, and inclemency of the weather, that out of 112 only 41 recovered; and yet that number is full ſufficient to prove his opinion of the diſeaſe, and of the uſe of inoculation.

Application was made, in 1770, to the Lord Preſident of the Council by a famous inoculator, for leave to take matter from the infected beaſts in Hampſhire, and to inoculate the cattle in the Southern and Weſtern counties of England: on a repreſentation to his lordſhip, that by ſuch an operation the contagion would not only be introduced in thoſe counties where it had not yet appeared, but alſo might ſpread the ſickneſs, ſo as to become general all over the kingdom as before, a poſitive and ſtrict injunction was given to drop the intention; eſpecially as by killing the cattle there was no doubt of extirpating the contagion out of Hampſhire. The inoculator therefore made no attempt.

According to the ſeveral prejudices of different countries, various opinions have ariſen of the nature of this

 ſickneſs.

ſickneſs. Such as are averſe to inoculation have obſtinately refuſed to acknowledge it was ſimilar to the ſmall-pox in the human body, and have very idly aſſerted, that the only intention of declaring this contagion to be a ſort of ſmall-pox was purpoſely, and with no other view than to promote inoculation for the ſmall-pox. Others have as poſitively declared it to be a peſtilential putrid fever, owing to a corrupted atmoſphere, and ariſing from infected paſtures; but unfortunately for the ſupporters of this opinion, while the contagious diſtemper raged with the utmoſt violence on the coaſts of Friſeland, North and South Holland, Zealand, and Flanders, there was not the leaſt appearance of it on the Engliſh Coaſt from the North Foreland to the Humber, although the coaſt and climate are the ſame.

I ſhall not dwell on Mr. TURBERVILLE NEEDHAM'S eloquent diſcourſe read at Bruſſels, ſince he muſt have been convinced, when he came to England in 1776, that the illneſs was of another ſort than he imagined; for ſuch a proof of the inefficacy of ſalt recommended by him as an antiſeptic in this diſeaſe has been given as is poſitive and deciſive; namely, that in Scania, a province in Sweden, where it is cuſtomary to place a large piece of rock ſalt, called *ſalt-ſtein*, for the cattle houſed in winter to lick, that they may be urged to drink, all the cattle in that

that province were feized with the contagious diftemper, and not one out-lived it. Monf. PAULET, in his *Recherches fur les Maladies Epizootiques*, vol. II. p. 25, 26. Paris 1776, has fufficiently exploded Mr. NEEDHAM's opinion.

M. BERGIUS had infifted, that the contagion was not of the exanthematous fort, and therefore inoculation muft be of no ufe; but this opinion was alfo fully refuted by the late Profeffor ERXLEBEN of Gottingen, in his learned oration on the 20 of October, 1770.

From every information, domeftic or foreign, and comparing the feveral opinions, experience and obfervation plainly and completely determine the difpute. The difeafe among the horned cattle, fo fatal in many countries, is not endemial or natural to Europe, although it is become fo in Denmark, from fpreading all over the Danifh dominions, and its long continuance in that kingdom. It is an eruptive fever of the variolous kind[(b)], and notwithftanding the exanthemata, or puftules, may have been frequently overlooked, yet none ever recovered without more or lefs eruption or critical abfceffes; but thefe differ from the peftilential fort; no otherways fimilar

(*b*) In a letter from Monf. VICQ D'AZYR to Dr. LAYARD, dated Paris, Auguft 28, 1780, is the following declaration: "Il me paroit comme à vous "que c'eft toujours la meme maladie qui a regné depuis 1711; et qu'elle a "de grands rapports avec l'eruption varioleufe."

lar to the plague, but, like unto the ſmall pox, it is communicated by contact, by the air conveying the effluvia, which alſo lodge in many ſubſtances, and are thereby carried to very diſtant places. Unlike other peſtilential, putrid, or malignant fevers, it bears all the characteriſtic ſymptoms, progreſs, criſis, and event of the ſmall-pox; and, whether received by contagion or inoculation, has the ſame appearances, ſtages, and determination, except more favourably by inoculation, and with this diſtinctive and deciſive property, that a beaſt having had the ſickneſs, naturally or artificially, never has it a ſecond time.

Thus, SIR, I have endeavoured to lay before you and the Royal Society, the reſult of my inquiries, experiments, obſervations, and correſpondence, concerning this calamitous ſickneſs, which, from my ſituation in Huntingdonſhire in 1756, it fell to my lot to inveſtigate.

His Majeſty's paternal care recommended from the Throne in 1770, and ordered every attention to be given to free his ſubjects from the impending calamity, which had been felt ſo ſeverely in former years. The great Council of the Nation gave the ſtrongeſt aſſurances of their readineſs to ſupport and aſſiſt the Royal commands; and the moſt ſalutary and effectual directions and orders, which originated in Great Britain were humanely and generouſly tranſmitted, by a conſtant correſpondence in

the courſe of above ten years, to every diſtreſſed ſtate abroad, who alſo have enjoyed the ſame ſucceſs.

You will allow me, SIR, I hope, to expreſs the peculiar ſatisfaction I feel as an Engliſhman, that my endeavours in this public ſervice are honoured with the approbation of our moſt gracious Sovereign, and the good-will of my native country.

And happy in this opportunity I requeſt you, SIR, to accept of my warmeſt wiſhes that you may long fill the high office of Preſident of the Royal Society, with health to purſue your philoſophical reſearches; and may enjoy from the moſt reſpectable and liberal Society in the world, the juſt and due tribute of their hearty acknowledgements for having their inſtitution, honour, and intereſt, ſo much at heart.

I have the honour, &c.

Lower Brook Street,
April 8, 1780.

XXXII. *An Investigation of the Principles of progressive and rotatory Motion.* *By the Rev.* S. Vince, *A. M. of* Sidney College, Cambridge. *Communicated by* George Atwood, *A. M. F. R. S.*

Read June 15, 1780.

THE communication of motion from impact is well known to constitute a considerable part of that branch of natural philosophy called mechanics; and as all our enquiries therein are directed, either to assist us in those operations which add to the conveniences of life, or to explain, for the satisfaction of the mind, those changes which we daily see arise from the effects of bodies on each other, it might naturally have been expected that the attention of philosophers would have been engaged, first in the investigation of such cases as most frequently occur from the accidental action of one body on another, before they had proceeded to others less obvious. A little consideration will convince any one how seldom it happens, in the collision of two bodies, that their centers of

of gravity and point of contact lie in the line of direction of the striking body, yet few writers on mechanics have extended their enquiries any further than this simple case. It must however be acknowledged, that the action of bodies on each other, in directions *not* passing through their center of gravity, affords a subject at least curious in speculation; for my own part, I have little doubt but that it might be rendered extremely useful to the practical mechanic. I. BERNOULLI was the first who published any thing on this subject. He found the point about which a body at rest would begin to revolve when struck by another body, observing however that D. BERNOULLI had also discovered the same: he has also mentioned the curve described by that point in the progressive motion of the body, and has directed a method of enquiry by which the velocities of the bodies may be found after the stroke, which comprehends all he has done on the subject. Two years afterwards D. BERNOULLI published a paper on progressive and rotatory motion, containing nothing more than what I. BERNOULLI had before given us, and, what is a little extraordinary, says in the introduction, *de tali quidem percussione nihil adhuc, quantum scio, publici juris factum fuit ab iis, qui de motu corporum a percussione egerunt*. EULER has also investigated the velocities of the

bodies after impact in a manner somewhat different, but has rendered it much more intricate by a fluxional calculus. To any one, however, who attentively considers the subject, the theory must still appear to be extremely imperfect, as, independent of principles not more self-evident than the propositions they are intended to demonstrate, which both I. and D. BERNOULLI have assumed in their investigations, a great variety of other circumstances equally interesting naturally arise in an enquiry into this matter, circumstances absolutely necessary towards understanding the principles of the motion of the bodies after impact. This induced me to consider the subject with some attention; and presuming that I have not been altogether unsuccessful in my endeavours to render the theory more perfect, I determined to lay the result of my enquiries before the Royal Society. I thought it expedient, for the sake of perspicuity, to divide the whole into distinct Propositions; and as the most simple cases are best understood, I have first considered the case of the action of a body on a lever having a corpuscle at each end: and I was the more induced to treat the subject in this manner, as most of the principles can be immediately applied to any number of corpuscles, in consequence of which the general investigations are rendered more easy and satisfactory.

PROP.

PROP. I.

Let A *and* B *be two indefinitely small bodies connected by a lever void of gravity, and suppose a force to act at any point* D *perpendicularly to the lever, to find the point about which the bodies will begin to revolve.*

From the property of the lever, the effect of the force acting at D (fig. 1.) on the body A is to the effect on B as BD : AD; hence the ratio of the spaces A*m*, B*n*, described by the bodies A and B in the first instant of their motion, will be as $\frac{BD}{A} : \frac{AD}{B}$; join *mn*, and if necessary produce that line and AB to meet in C, which will manifestly be the point about which the bodies begin to revolve. Hence from similar figures BC : AC :: $\frac{AD}{B}(\propto Bn) : \frac{BD}{A}(\propto Am) :: A \times AD : B \times BD$, or $DC - DB : AD + DC :: A \times AD : B \times BD$, and consequently $DC = \frac{A \times AD^2 + B \times BD^2}{B \times BD - A \times AD}$, and therefore D is the center of percussion or oscillation to the point of suspension C.

Cor. 1. Hence, whatever be the magnitude of the stroke at D, the point C will remain the same.

Cor. 2. If the force acts at the center of gravity G, the bodies will have no circular motion, for in this case $B \times BD - A \times AD = 0$, and therefore DC becomes infinite.

Cor. 3. If the force acts at one of the bodies, the center of rotation C will coincide with the other body.

Cor. 4. If the lever had been in motion before the stroke, the point C, at the instant of the stroke, would not have been disturbed.

PROP. II.

Let a given quantity of motion be communicated to the lever at D, *to determine the velocity of the center of gravity* G.

The space Am, described by the body A in the first instant of motion, is as $\frac{DB}{A}$; now $CG = CD - DG = CD - AG + AD = \frac{A \times AD^2 + B \times BD^2}{B \times BD - A \times AD} - AG + AD = \frac{B \times BD \times BG + A \times AD \times AG}{B \times BD - A \times AD}$; also $CA = CD + DA = \frac{A \times AD^2 + B \times BD^2}{B \times BD - A \times AD} + DA = \frac{B \times BD \times AB}{B \times BD - A \times AD}$; hence we have $\frac{B \times BD \times AB}{B \times BD - A \times AD}$ (AC) : $\frac{BD}{A}$ ($\propto m$A) :: $\frac{B \times BD \times GB + A \times AD \times AG}{B \times BD - A \times AD}$ (CG) : $\frac{B \times BD \times GB + A \times AD \times AG}{A \times B \times AB} \propto G\omega$ the velocity of the center of gravity; hence if the motion be communicated at G, the velocity becomes as $\frac{B \times GB^2 + A \times AG^2}{A \times B \times AB}$.

Let now the motion, which is supposed to be actually communicated to the rod at D, be equivalent to the motion of a body whose magnitude is G, and moving with a velocity v; then if that motion be communicated at G, the velocity of the center of gravity is well known to be

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$= \frac{G \times v}{A+B}$; hence $\frac{B \times BG^2 + A \times AG^2}{A \times B \times AB} : \frac{B \times BD \times BG + A \times AD \times AG}{A \times B \times AB} ::$ $\frac{G \times v}{A+B} : \frac{G \times v}{A+B} \times \frac{B \times BG \times BD + A \times AD \times AG}{B \times BG^2 + A \times AG^2} =$ the velocity of the center of gravity, when the ſame motion is actually communicated to any point D. Now $BD = BG + GD$, and $AD = AG - GD$; hence $B \times BG \times BD + A \times AD \times AG = B \times BG^2 + A \times AG^2 + GD \times \overline{B \times BG - A \times AG} =$ (becauſe $B \times BG - A \times AG = 0$) $B \times BG^2 + A \times AG^2$; conſequently the velocity becomes $\frac{G \times v}{A+B}$; and hence the center of gravity moves with the ſame velocity, wherever the motion is communicated.

PROP. III.

Let a given elaſtic body P, *moving with a given velocity, be ſuppoſed to ſtrike the lever at the point* D *in a direction perpendicular to it; to determine the velocity of the center of gravity* G *after the ſtroke.*

Suppoſe firſt the body to be non-elaſtic, and let v be the velocity of the center of gravity after the ſtroke upon that ſuppoſition, and V the velocity of the ſtriking body: then: CG : CD :: $v : \frac{v \times CD}{CG} =$ the velocity of the point D after the ſtroke, or of the body P; for the ſame reaſon $\frac{v \times CA}{CG}$ and $\frac{v \times CB}{CG}$ equal the velocities of A and B reſpectively. Now, becauſe in revolving bodies, the

the momenta, arising from the magnitude of the bodies, their distance from the center of rotation and velocity conjointly, remain the same after the stroke as before, we shall have $P \times V \times DC = \frac{v \times CD^2 \times P}{CG} + \frac{v \times CA^2 \times A}{CG} + \frac{v \times CB^2 \times B}{CG}$, and therefore $v = \frac{P \times V \times DC \times CG}{P \times DC^2 + A \times AC^2 + B \times BC^2} = \frac{P \times V \times CG}{\overline{A+B} \times CG + P \times DC}$; hence if P be supposed an elastic body, we shall have $\frac{2 \times P \times V \times CG}{\overline{A+B} \times CG + P \times DC}$ for the velocity of the center of gravity after the stroke, *in ipso motûs initio.*

PROP. IV.

To determine the motion of the bodies after the first instant, or when they are left to move freely by themselves.

The writers on mechanics, from considering the equality of motion on each side the center of gravity, when a body revolves about that point, have inferred, that if a body had a projectile as well as a circular motion communicated to it, the center of gravity would continue to move in a right line, as that point would not be disturbed by the rotatory motion: yet, as, in the case we are now considering, the bodies begin to revolve about a different center, it may be proper to examine more accurately into this matter, and to shew from what

what principle it is that the motion of the center of gravity is preserved in a right line.

Let a motion perpendicular to the rod be communicated to A (fig. 2.) and then by Cor. 3. Prop. I. B will not be disturbed by such an action; and A will in the first instant have a tendency to revolve about B as a center, and would actually describe the arc AH, if the body B were fixed: let the angle ABH be supposed infinitely small, and let GK be the arc, the center of gravity would have described, and draw the tangents AF, G*g* to the arcs AH, G*g* respectively. Now, if A could have moved freely, it would (because AF = AH) have described AF in the same time the arc AH was described, upon supposition that B was fixed; for the radius BA being perpendicular to the circular arc AH, the force of the lever could have no efficacy to accelerate or retard the motion of A in the arc AH, and therefore the velocity in that arc is the same as it would have been if it had moved freely in the tangent: hence HF is that space through which the centrifugal force of A would have carried that body, could it have moved freely; but as A is connected to B by means of the lever, it is manifest that the same force which would have carried A from H to F in the direction of the lever, will, when it has both bodies to move, carry it over a space which is to FH as A : A + B,

or

or as Bg : BH, or as gK : BH; hence that space, or the space through which the centrifugal force of A will draw the lever in the direction BH, is equal to Kg; that is, the point K, which is the center of gravity of A and B, will be found at g, and consequently the center of gravity has preserved its motion uniform in the right line Gg, inasmuch as the centrifugal force, acting perpendicularly to the direction of the center of gravity, can neither accelerate or retard its motion. In the same manner it may be proved, that the motion of the center of gravity is continued uniform in the same right line, whatever be the position of the lever. Moreover, as the centrifugal force acts in the direction of the lever, it cannot alter its angular velocity, which will therefore remain as *in ipso motûs initio*. If now we suppose that to the force impressed upon A, two other equal accelerative forces be communicated to A and B at the same time, it is evident that no alteration can arise from the actions of the bodies on each other; and the case will then be similar to the motion of the bodies, supposing a single force had been impressed at any point D. The like method of reasoning may be extended to any number of bodies.

The same thing may also easily be demonstrated in the following manner. The centrifugal forces of A and

and B (fig. 1.) are respectively $A \times AC$ and $B \times BC$; also the centrifugal force of the point G, considering it as having both bodies to move in the direction of the rod, is $\overline{A+B} \times GC$, but from mechanics $A \times AC + B \times BC = \overline{A+B} \times GC$: hence the centrifugal forces of the bodies A and B give the center of gravity a centrifugal force equivalent to its own centrifugal force, which, as the latter would cause that center to move in the tangent G*g*, the lever not being fixed at C, it is manifest that the former will cause the center of gravity to continue its motion in the same direction.

That this motion of the lever, in a direction from the center C, is the only motion which is communicated to it from the effect of the bodies A and B is manifest from hence. The bodies begin to revolve freely about the point C, and consequently if the point C had been fixed, the bodies would have moved on with a uniform angular velocity about C; if therefore we suppose the lever not to be fixed at C, as the efficacy of the centrifugal force which acts in the direction of the lever is now suffered to take place, and no new external force is impressed on either of the bodies, it is manifest, that if in the former case the bodies had no efficacy to disturb the angular velocity of the lever, they cannot have any in the latter, consequently the angular velocity, and from what

 has

has been before proved, the uniform motion of the center of gravity in a right line, remain unaltered, after the commencement of the motion.

PROP. V.

In the time the bodies make one revolution, the center of gravity will move over a ſpace equal to the circumference of a circle whoſe radius is CG (*fig.* 1.).

From the laſt Propoſition, the angular velocity of the lever is continued uniform; hence the time of a revolution is juſt the ſame as if the point C were fixed, and the bodies were to continue to revolve about that point as a center, in which caſe the center of gravity G, in the time of a revolution, would evidently deſcribe the circumference of a circle whoſe radius is GC. This therefore is the ſpace the center of gravity deſcribes in a right line when the bodies move freely, for from the laſt Propoſition that center is carried uniformly forward with the ſame velocity.

Cor. 1. Hence if the magnitude of the force acting at D vary, the velocity of the center of gravity will vary in the ſame ratio as the angular velocity.

Cor. 2. Hence the point D may be found, where a force being applied, the bodies ſhall make one revolution,

tion, whilst the center of gravity moves over any given space (s): for let p = the periphery of a circle whose radius is unity, then $p : 1 :: s : \frac{s}{p}$ = the radius of a circle whose circumference is the space to be passed over in the time of a revolution, and which must therefore, by the Proposition, be equal to GC; the point C therefore being determined, D may be easily found, for from mechanics $CG \times DG$ is given; and from Cor. 3. Prop. I. when D comes to A, C will coincide with B, ∴ $CG \times GD = AG \times GB$, and consequently $DG = \frac{AG \times GB}{CG}$.

PROP. VI.

To determine the time of one revolution, supposing every thing given as in Prop. III.

The point D being given, we have from Cor. 2. to the last Proposition, $CG = \frac{AG \times GB}{DG}$; put W equal the circumference of a circle whose radius is CG, and it appears from the last Proposition, that W is the space the center of gravity passes over in the time of one revolution; hence, because from Prop. IV. the center of gravity moves uniformly, we have by Prop. III. $\frac{2 \times V \times P \times CG}{\overline{A+B} \times CG + P \times DC}$: 1″

$::1'' :: w : w \times \frac{2 \times V \times P \times CG}{\overline{A+B} \times CG + P \times DC}$ = the time of one revolution.

Cor. Hence the angular velocity being inversely as the time of a revolution, will vary as $\frac{\overline{A+B} \times CG + P \times DC}{V \times P \times CG \times W}$.

PROP. VII.

The point C, *as the center of gravity moves forward, will describe the common cycloid.*

From the description of the common cycloid it appears, that the center of the generating circle passes over a space equal to the circumference of that circle whilst it makes one revolution. With the center G (fig. 3.) and radius GC, describe the circle C*xy*, and draw CR, GW perpendicular to ABC, and let the circle C*xy* be supposed to revolve on the line CR; then will the center G move over a space equal to the circumference of the circle C*xy* whilst it makes one revolution, and the point C will describe the common cycloid: but from Prop. V. the point G *will* move over a space equal to the circumference of a circle whose radius is GC, whilst the bodies, and consequently GC, make one revolution; and hence the point C will describe the same curve as before, that is, the common cycloid.

PROP.

PROP. VIII.

Let a motion be communicated to the lever obliquely, to determine the point about which the bodies begin to revolve.

Let FD (fig. 4.) represent the force communicating the motion at the point D, which resolve into two others FH, HD, the former FH parallel to the lever, and the latter HD perpendicular to it. Let C be the point about which the bodies would have begun to revolve, had the force HD only acted, and which may be found by Prop. I.; and suppose in this case *mgn* to have been the next position of the lever after the commencement of the motion, or that the bodies A, B, and center of gravity G, had been carried to *m*, *g* and *n* respectively. But as the force FH acts at the point D at the same time in the direction of the rod, if we take G*q* : G*g* as FH : HD, then whilst the center of gravity would have moved from G to *g* in consequence of the force HD, it will by means of the force FH be carried in the direction of the lever from G to *q*, and also every other point of the lever will be carried in the same direction with the same velocity; take therefore A*p* and B*r* each equal to G*q*, and complete the parallelograms A*a*, G*w* and B*b*, and the bodies A, B, and center of gravity G will, at the end of that time, be

found

found at *a*, *b* and *w* refpectively, and *awb* will be the pofition of the lever. Now it is evident, that C is not the point about which the bodies begin to revolve, for (confidering the lever to be produced to C) that point muft have moved over a fpace C*c* equal to G*q*, when the lever is come into the pofition *awb*: draw *c*O perpendicular to CB, and GO perpendicular to G*w*, and O will be the center of rotation at the commencement of the motion. For conceive CO to be a lever, then the lever ABC has a circular motion about C, whilft that point is moving from C to *c*, and confequently the point O is carried forward in a direction parallel to C*c* by this motion; but as the lever CO is carried by a circular motion about C in a contrary direction, it is evident that that point of the lever CO muft be at reft where thefe two motions are equal, as they are in contrary directions. Now the velocity of C in the direction C*c* : velocity of G about C :: G*q* : G*g* :: (by fim. triang.) CO : CG, and the velocity of the point G about C : velocity of the point O about C :: CG : CO; hence *ex æquo* the velocity of C in the direction of C*c*, or of O in the direction of parallel to C*c*, is equal to the velocity of the fame point O in a contrary direction arifing from its rotation about C, and confequently O being a point at reft, muft be the center of rotation *in ipfo motûs initio*. Alfo, becaufe *ma* is equal

and

and parallel to nb, ub must be equal and parallel to mn, therefore the angular velocity is just the same as if the force FH had not acted. The center O of rotation at the beginning of the motion being thus determined, every thing relative to the motion of the bodies, after they are at liberty to move freely, may be determined as in the preceding Propositions.

Cor. 1. Hence it appears, that whatever be the magnitude or direction of the force communicating the motion, or the point at which it acts, the center of gravity will move in a line parallel to the direction of the force, for the triangles FHD, Gqw being similar, Gw must be parallel to FD.

Cor. 2. The same is manifestly true for any number of bodies; for let (fig. 5.) E be a third body, and conceive it to be connected with the other two bodies A and B in their center of gravity G; then if FD represents the force acting at the point D, it is evident from the last Corol. and the second Prop. that the center of gravity moves with the same velocity and in the same direction, as if the same motion had been communicated at G in a line RG parallel to FD, and that the center of gravity has the same velocity communicated to it, as if the two bodies had been placed at G; conceive therefore the bodies A and B to be placed at G, and let the force act at D, and

then from the last Corol. the center of gravity g, of the three bodies, will move in a line parallel to the direction of the force communicated. In the same manner it may be proved for any number of bodies.

SCHOLIUM.

The method here made use of to determine the point of rotation *in ipso motûs initio*, when a single force acts at any point D, may be applied, when any number of forces act at different points at the same time. For let (fig. 1.) α, β, γ, &c. represent the forces acting on the lever at the points D, E, F, &c. respectively; then from the same principles the effect of all the forces on A : the effect on B : : $\frac{\alpha}{AD} + \frac{\beta}{AE} + \frac{\gamma}{AF} + \&c. : \frac{\alpha}{BD} + \frac{\beta}{BE} + \frac{\gamma}{BF} + \&c.$ which quantities put equal to P and Q respectively, and then $\frac{P}{A} : \frac{Q}{B} : : Am : Bm : : AC : BC$, from whence it appears, that (putting $GC + GA = AC$ and $GC - GB = BC$) the distance $GC = \frac{A \times Q \times AG + B \times P \times BG}{B \times P - A \times Q}$. The same conclusion might have been deduced from this consideration; that if any number of forces act on a lever, the effect on any point of that lever is just the same as if a force, equivalent to the sum of these forces, had

had acted at their common center of gravity, find therefore their common center of gravity, and conceive a force equivalent to them all to be communicated to that point, and the Problem is reduced to the case of the first Proposition. If any of the forces had acted on the opposite side of the lever, such forces must have been considered as negative.

If there be any number of bodies placed on the lever, and a single force acts at D, it will appear from the same principles that the point C, about which they begin to revolve, will be the point of suspension to the center of percussion D; and the same conclusion will be obtained, if the bodies be not situated in a straight line. As a direct investigation, however, is always to be preferred to conclusions drawn from induction, it may be thought proper, before we apply any of the foregoing principles to the case of the action of bodies upon each other by impact, to shew how such a direct investigation to determine the point about which a body, having a motion communicated to it, begins to revolve, may be obtained; previous to which, however, some further considerations are necessary.

PROP. IX.

If a force acts upon a body in any given direction not passing through the center of gravity; to determine the plane of rotation, the direction in which the center of gravity begins to move, and its motion after.

Conceive a plane AYBZ (fig. 6.) to be supported upon a line AB passing through its center of gravity G, and suppose a force to act at any point D in that line, and in a direction perpendicular to the plane; then it is manifest, that such a force can give the plane no rotatory motion about AB. Imagine now the support to be taken away whilst the force is acting at D, then it is evident, that as the plane had no tendency to move about AB as an axis, and the taking away of the support can give it no such motion, it will, by Cor. 2. Prop. VIII. begin its progressive motion in the direction in which the force acts; and as the force is supposed not to act at the center of gravity, it must at the same time have a rotatory motion about some axis, which, as it has no motion about AB, must lie somewhere in the plane, and perpendicular to AB; and consequently *in ipso motûs initio* the plane of rotation must be perpendicular to the plane

AYBZ.

AyBZ. Let LCM, perpendicular to AB, be the axis about which the plane begins to revolve, and p, q be two equal particles of the plane similarly situated in respect to AB, also qb, pa perpendicular to LCM. Now the centrifugal force of p, or its force in the direction ap is $p \times ap$, and that of q in the direction bq is $q \times bq$; to determine now how these forces will affect the motion of the plane, we may observe in the first place, that the force $p \times ap$, acting at a in the plane, must tend to give it a motion about an axis perpendicular to the plane; but as an equal force $q \times qb$ acts at q to give it a motion in a contrary direction, it is evident that the two forces will destroy each other, so far as they tend to generate any motion in the plane about an axis perpendicular to it; and hence it is manifest, that if the parts of the plane AyB, AZB, be similar, and similarly situated in respect to AB, the plane, after the commencement of the motion, will have no tendency to revolve about an axis perpendicular to it. Also, as the centrifugal force of each particle acts in a direction parallel to AB, it can give the plane no tendency to revolve about that line as an axis, and consequently the plane of rotation will be preserved as *in ipso motûs initio*. Conceiving therefore the plane on each side the line AB to be similar, and similarly situated, suppose another plane to be fixed upon this, whose parts

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on each side AB are similar, and similarly situated, and the force to act as before, then it is manifest, that as each plane endeavours to preserve the same plane of rotation, the two planes connected will also continue to move in the same plane of rotation, for the action of one plane on another, on each side the plane of rotation, being equal, cannot tend to disturb the motion in that plane; and as this must be true for any number of planes thus similar and similarly situated, it is evident, that if a force should act upon a body, and each section, perpendicular to the direction of the force, should be similar on each side the plane passing through the direction of the force, and the center of gravity of the body, that that plane would be the plane of rotation in which the body would both begin and continue its motion. It appears also from what has been proved, that if every section on each side that plane had not been similar, the plane of rotation would not *necessarily* have continued the same after the commencement of the motion. Hence all bodies, formed by the revolution of any plane figure, will have the axis about which they were generated, a fixt axis of rotation; to determine, however, every other axis of a body about which it would continue to revolve, would be foreign to the subject of this paper. Supposing therefore the plane of rotation to continue the same

same (for in this paper I mean to confine my enquiries to such cases) imagine all the particles of the body to be referred to that plane orthographically, which supposition not affecting the angular motion of the body, the centrifugal force of all the particles, to cause the body to revolve about an axis perpendicular to that plane, will remain unaltered. Let LMNO (fig. 7.) be that plane, and suppose a force to act at A in the direction PA lying in the same plane, which produce until it meets LN, passing through the center of gravity G, perpendicularly in D; then by Cor. 2. Prop. VIII. the center of gravity G will begin its motion in a line parallel to PA, or perpendicular to LN; and consequently the center C, about which the body begins to revolve, must lie somewhere in the line LN. Now the centrifugal force of any particle p is $p \times pc$; let fall pa perpendicular to LN, then the effect of that force at C, in a direction perpendicular to LN, will be $p \times pa$, and in the direction CL it will be $p \times ca$; but as the sum of all the quantities $p \times pa = 0$, and the sum of all the quanties $p \times ca =$ the body multiplied into CG, it follows from the same reasoning as in Prop. III. that the point G will continue to move in a direction perpendicular to LN; and also, as the forces $p \times ca$ act in a direction perpendicular to that in which the center of gravity moves, its motion must be conti-

nued

nued uniform. In the following Propositions, therefore, we suppose the axis of the body, after the commencement of the motion, to continue perpendicular to the plane passing through the direction of the force, and the center of gravity of the body, and that the body itself is orthographically projected upon that plane; also in the case of the action of two bodies on each other, the plane passing through the direction of the striking body and point of percussion is supposed to pass through the centers of gravity of each body; that the axis of each body after it is struck continues perpendicular to that plane, and that each body is reduced to it in the manner above described.

PROP. X.

To determine the point about which a body, when struck, begins to revolve.

Let LMNO (fig. 7.) represent the body, G the center of gravity, and PA the direction of the force acting at A, which produce till it meets LN, passing through G, perpendicularly in the point D; draw *pb* perpendicular to *p*C, on which (produced if necessary) let fall the perpendicular D*w*; C being supposed the point about which the body begins to revolve, and which, from the last Proposition, is somewhere in the line LN. Because the body, in consequence

sequence of the force acting at D, begins to revolve about C, and consequently if immediately after the beginning of the motion a force were applied at D equal to it, and in a contrary direction, the motion of the body would be destroyed, it is evident, that the efficacy of the body revolving about C, to turn the body about D, should any obstacle be opposed to its motion at that point, must be equal to nothing; for were it not, the body, when stopped at D, would still have a rotatory motion about that point, and consequently two equal and opposite forces applied at D would not destroy each others effects, which would be absurd. Now the force of a particle p, in the direction pw, being $p \times pC$, its efficacy to turn the body about the point D is $p \times pC \times Dw$; but by sim. triang. $Dw : Db :: aC : pC$, $\therefore Dw = \frac{Db \times aC}{pC}$, and consequently the efficacy to turn the body about $D = p \times Db \times aC = p \times Ca \times \overline{DC - Cb} = p \times Ca \times DC - p \times pC^2$; hence the sum of all the $p \times Ca \times DC$ − the sum of all the $p \times PC^2 = 0$, and consequently $CD = \frac{\text{sum of all the } p \times PC^2}{\text{sum of all the } p \times Ca}$, therefore D is the center of percussion, the point of suspension being at C.

Cor. From this and the preceding Proposition it appears, that every thing which was proved in Prop. V. VI. VII. holds here also in the case of the action of one body on another.

PROP.

PROP. XI.

Let a body P (fig. 8.) *moving with the velocity* V, *strike the body* Q *at rest in the point* A, *and in a direction* AD *passing through the center of gravity of the striking body; to determine the velocity of each body after the stroke, supposing them to be elastic.*

The solution of this Proposition depending on the same principles as that of Prop. III. we shall have, putting v equal the velocity of the center of gravity G after the stroke, on supposition that the bodies were non-elastic (DGC being supposed perpendicular to AD, and C the point about which the body Q begins to revolve) $V \times P \times CD = \frac{v \times P \times CD^2}{CG} + \frac{v \times \text{sum of all the } p \times Cp^2}{CG}$, and consequently $v = \frac{V \times P \times CD \times CG}{\text{sum of all the } p \times pC^2 + P \times CD^2}$; but it is well known, that the sum of all the $p \times pc^2 = CG \times CD \times Q$, and hence $v = \frac{V \times P \times CG}{Q \times CG + P \times DC}$, and therefore if the bodies be supposed elastic, we have $\frac{2P \times V \times CG}{Q \times CG + P \times DC}$ for the velocity of the center of gravity G after the stroke. Now to determine the velocity of P, we have $\frac{P \times V \times CD}{Q \times CG + P \times DC}$ equal its velocity after the stroke from single impact, and consequently $V - \frac{P \times V \times CD}{Q \times CG + P \times DC} = \frac{Q \times V \times CG}{Q \times GC + P \times DC}$ is the velocity lost

loſt by P from ſimple impact; hence if the bodies be elaſtic, $\frac{2\times Q\times V\times CG}{Q\times GC+P\times DC}$ will be the velocity loſt by P if elaſtic, and conſequently the velocity of P after the ſtroke $= V - \frac{2\times Q\times V\times CG}{Q\times GC+P\times DC} = \frac{P\times DC-Q\times GC}{Q\times GC+P\times DC}\times V$.

Cor. 1. If the direction AD paſſes through G, then CG being equal to CD, we have $\frac{2PV}{Q+P}$ = Q's velocity, and $\frac{P-Q}{P+Q}\times V$ = P's velocity, which is well known from the common principles of elaſtic bodies.

Cor. 2. If $P\times DC = Q\times GC$, or $P : Q :: GC : DC$, then will the body P be at reſt after the ſtroke.

Cor. 3. If Q were infinitely great, the velocity of P after the ſtroke would be $= -V$ as it ought, for P would then ſtrike againſt an immoveable obſtacle.

Cor. 4. Whatever motion Q gains from the action of P, it would loſe, if, inſtead of ſuppoſing P to ſtrike Q, Q were to move in an oppoſite direction, and ſtrike P at reſt with the ſame velocity with which P ſtruck Q; in ſuch caſe, therefore, the velocity of Q after the ſtroke would be $V - \frac{2P\times GC\times V}{Q\times GC+P\times DC} = \frac{\overline{Q-2P}\times GC+P\times DC}{Q\times GC+P\times DC}\times V$.

Cor. 5. Hence if P be infinitely great, or Q be ſuppoſed to ſtrike an immoveable object, its velocity after the ſtroke will be $= \frac{DC-2CG}{DC}\times V$: hence when $DC=2GC$, the body Q will have no progreſſive motion after the ſtroke,

ſtroke, but would in ſuch caſe, if P were immediately taken away, continue to revolve about a fixed axis. It may alſo be obſerved, that when DC is greater than 2GC, or the velocity of Q is poſitive, that, becauſe it is impoſſible for Q to continue its progreſſive motion, it is only to be underſtood, that if immediately after the impact the body P were removed, the body Q would then proceed with ſuch a velocity.

Cor. 6. Suppoſe the bodies to be non-elaſtic, and let M be the magnitude of a body placed at D, which, being acted upon by P, ſhall have the ſame velocity generated as was before generated in the point D of the body Q; then by the common rule for non-elaſtic bodies, the velocity of M after the ſtroke will be $\frac{P \times V}{P+M}$, and hence $\frac{P \times V}{P+M} = \frac{P \times V \times DC}{Q \times CG + P \times DC}$, conſequently $M = Q \times \frac{GC}{DC}$.

Cor. 7. If a given quantity of motion were communicated to *any* point of the body Q, the progreſſive motion of that body after the ſtroke would be the ſame. For ſuppoſe the magnitude of the body P to be diminiſhed *ſine limite*, and its velocity to be increaſed in the ſame ratio, then, becauſe $\frac{P \times V \times CD}{Q \times CG + P \times DC}$ (which is the velocity of P after the ſtroke, if the bodies be non-elaſtic) = (becauſe P is infinitely ſmall) $\frac{P \times V \times CD}{Q \times CG}$, the velocity of P after the

ſtroke

ſtroke from ſimple impact is finite, conſequently its motion muſt be infinitely ſmall, and therefore P muſt have communicated all its motion to Q: now in this caſe the velocity of Q $(= \frac{P \times V \times CG}{Q \times CG + P \times CD}) = \frac{P \times V}{Q}$, which quantity is independent of the place where the force acts; in the ſame manner it would appear if we had ſuppoſed the bodies elaſtic.

PROP. XII.

Suppoſing every thing given as in the laſt Propoſition, except that the direction AD *does not paſs through the center of gravity g of the ſtriking body; to determine the velocity of each body after the ſtroke.*

Let AD (fig. 9.) be produced to meet F*g*O paſſing through *g*, the center of gravity of the ſtriking body, perpendicularly in F, and ſuppoſe O to be the point of the body P which is not diſturbed by the action of P on Q: now it appears from Cor. 6. Prop. XI. that if both bodies were non-elaſtic, and a body equal to $Q \times \frac{CG}{CD}$ were placed at D, the velocity of that body, from the action of P, would be equal to the velocity of the point D of the body Q; for the ſame reaſon, therefore, it appears, that if, inſtead of ſuppoſing

P to ſtrike Q in the direction FA, a body equal to $P \times \frac{GO}{FO}$ were to ſtrike Q at the ſame point and in the ſame direction (which direction is ſuppoſed to paſs through the center of gravity of that body) the effect on Q would be the ſame; hence, if in the quantity $\frac{V \times P \times CD}{Q \times GC + P \times DC}$, which from the laſt Prop. expreſſes the velocity of the point D after the ſtroke, on ſuppoſition that the bodies are non-elaſtic, we ſubſtitute for P a body equal to $P \times \frac{GO}{FO}$, we ſhall have $\frac{V \times P \times DC \times gO}{Q \times GC \times FO + P \times gO \times DC}$ for the velocity of the point D from the action of P; and conſequently $\frac{2 \times V \times P \times GC \times gO}{Q \times GC \times FO + P \times gO \times DC}$ = the velocity of the center of gravity G of the body Q, after the ſtroke, if the bodies be perfectly elaſtic. To determine now the velocity of the ſtriking body, let of, perpendicular to og, be the ſpace deſcribed by the point O in the firſt inſtant of time after the ſtroke, which, as that point is not diſturbed by the action of the bodies on each other, may repreſent the velocity of P before the ſtroke, and let Fb repreſent the velocity of the point F after the ſtroke; join fb, and draw gd perpendicular to og, and then will gd repreſent the velocity of the center of gravity g of the ſtriking body after the ſtroke. Draw fc perpendicular to FA, and produce gd to meet fc in e; now the velocity

loſt

loſt by P at the point F by ſimple impact being equal to $V - \frac{V \times P \times DC \times gO}{Q \times GC \times FO + P \times gO \times DC} = \frac{V \times Q \times GC \times FO}{Q \times GC \times FO + P \times gO \times DC}$, we ſhall have *bc* the velocity loſt by the point F, on ſuppoſition that the bodies are perfectly elaſtic (ſuppoſing *of* to repreſent the value of V) equal to $\frac{2 \times V \times Q \times GC \times FO}{Q \times GC \times FO + P \times gO \times DC}$, and therefore by ſim. triang. *fc* (FO) : *cb* : : *fe* (O*g*) : *ed* = $\frac{2 \times V \times Q \times GC \times gO}{Q \times GC \times FO + P \times gO \times DC}$ = the velocity loſt by the center of gravity *g*, and hence $V - \frac{2 \times V \times Q \times GC \times gO}{Q \times GC \times FO + P \times gO \times DC} = \frac{V \times Q \times GC \times FO + V \times P \times gO \times DC - 2 \times V \times Q \times GC \times gO}{Q \times GC \times FO + P \times gO \times DC}$ = the velocity of P after the ſtroke. Now, as it appears from Prop. IX. that the progreſſive motion of a body, when left to move freely, continues uniform and in the ſame direction, it follows, that the expreſſions for the velocities of each body in the firſt inſtant after the ſtroke, both in this and the preceding Propoſition, will repreſent the uniform progreſſive velocities with which the bodies will continue to move, and conſequently the place of each body, at the end of any given time after impact, may eaſily be determined.

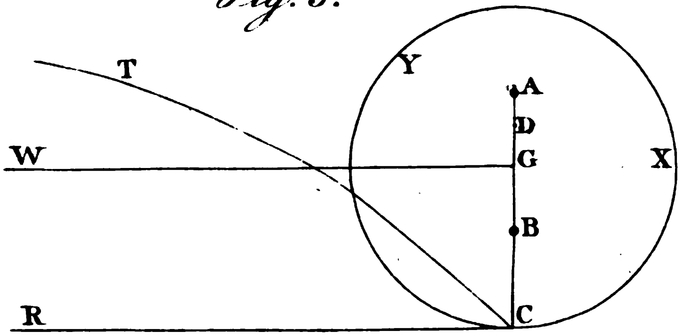
Cor. 1. If the direction FA paſſes through *g*, then FO and *g*O becoming infinite, we ſhall have $\frac{2 \times V \times P \times GC}{Q \times GC + P \times DC}$ for the velocity of Q, and $\frac{V \times P \times DC - V \times Q \times GC}{Q \times GC + P \times DC}$ for the velocity

velocity of P, agreeable to what was proved in the last Proposition.

Cor. 2. Hence the point about which P begins its rotatory motion may easily be found; for produce (if necessary) fb and OF to meet in a, and a will be the point required; and by sim. triang. $bc \left(= \frac{2\times V\times Q\times GC\times FO}{Q\times GC\times FO+P\times gO\times DC}\right)$: cf :: fo (= V) : $oa = \frac{Q\times GC\times FO+P\times gO\times DC}{2\times Q\times GC}$, and hence $Fa = \frac{P\times gO\times DC-Q\times GC\times OF}{2\times Q\times GC}$.

Cor. 3. If, instead of supposing Q to have been at rest, it had been moving forward in a direction parallel to that of the body P, with the velocity v, the motion of each body after the stroke may easily be determined: for considering P as acting upon Q with the velocity $V-v$, we shall have by this Proposition (putting $2M = \frac{2P\times GC\times GO}{Q\times GC\times FO+P\times gO\times DC}$) $\overline{V-v}\times 2M =$ the velocity communicated to G, therefore $v+\overline{V-v}\times 2M =$ the velocity of Q after the stroke: also $\overline{V-v}\times M\times\frac{CD}{CG} =$ the velocity gained by the point D from simple impact, and consequently the velocity of that point after $= v+\overline{V-v}\times M\times\frac{CD}{CG}$, hence $V-v-\overline{V-v}\times M\times\frac{CD}{CG} =$ the velocity lost by P at the point F from simple impact, therefore P's velocity after the stroke $= V-\overline{V-v-\overline{V-v}\times M\times\frac{CD}{CG}}\times\frac{2gO}{FO}$. In the same

Fig. 3.



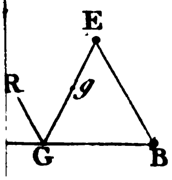


Fig. 6.

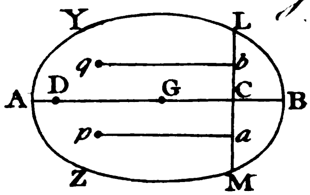


Fig. 8.

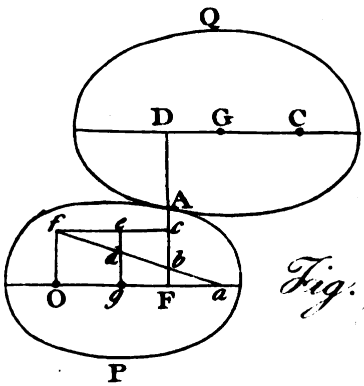


Fig. 9.

BASIRE sc.

manner it might have been determined, had Q moved in an opposite direction.

Cor. 4. Hence also we may easily determine the motion of each body after the stroke, supposing Q had not been moving in a direction parallel to the motion of P, by resolving Q's motion into two parts, one parallel to the motion of P, and the other perpendicular; and finding by the preceding what would be the effect of the parallel motions, and then compounding Q's motion, after the stroke from that consideration, with the motion it had in a direction perpendicular thereto before the stroke.

Cor. 5. The point *a* of the body P will describe (when that body after the stroke has any progressive motion) the common cycloid.

Cor. 6. Hence, therefore, the times of the revolutions of each body may be determined as in Prop. VI.

Cor. 7. If the bodies had any rotatory motion before impact, every thing relative to the motion of the bodies after the stroke might have been determined from the same principles.

XXXIII. *Continuation of the Case of* James Jones. *By* Richard Browne Cheston, *Surgeon to the* Gloucester *Infirmary. Communicated in a Letter to Mr.* Henry Watson, *Surgeon to the* Westminster *Hospital.* (*See p.* 323.)

Read November 16, 1780.

DEAR SIR,

I HAVE it at last in my power to inform you of the state the bones of the *pelvis* appeared in after a *maceration* of five months: for though by very seldom changing the water, and keeping the vessel containing it rather in a warm place, I suffered the highest putrefaction to come on, it took up that space of time before the soft parts were entirely destroyed.

In my account of the state in which I found the *thoracic duct* of the same subject, I mentioned my suspicions (from the only examination I then had it in my power to make) that a considerable part of the substance of the *os innominatum* was destroyed or absorbed; but *maceration* has cleared up this circumstance, and shewn, that the depth the probe entered, and the gritty resistance

resistance I felt in the body of the tumor, was not from its passing through a carious or diseased part, as there was reason then to suppose, but from the quantity of osseous matter deposited on the outward side of the *os innominatum*; and that the part so loaded with it externally, and as it afterwards proved to be *internally*, was apparently in a sound state.

It is difficult to fix upon the precise part where the tumor occupying the right moiety of the *pelvis*, and containg the boney matter, originated. There is, however, every reason to suppose it began low down in the *pelvis* under the *peritoneum*, as the surface of the tumor was evidently covered with that membrane; and from every circumstance in the appearance of the tumor, as well as a careful examination of it, during the progress of its dissolution, it is probable, that passing out of the *pelvis* anteriorly through the *foramen ovale*, as well as laterally under the *ischium*, and rising upwards over the spine of the *ilium*, it formed a complete communication from within outwardly, so as to envelope the whole of the right *os innominatum* entirely.

As the soft parts of the tumor decayed in maceration, great quantities of boney matter in irregular forms and of different sizes were found in the water at the bottom of the pan; and as no force, nor even

 motion,

motion, had been uſed which could have ſeparated this matter from what remained adherent to the bone, it is highly probable it was ever depoſited in, and diſperſed through, the tumor in a detached ſtate.

The tumor, externally, bore the uſual appearances of a diſeaſed or enlarged gland; but, by degrees) as I had opportunities to obſerve upon changing the water) the whole appearance was changed, and the boney matter, as the maceration proceeded, ſeemed ſurrounded by a hard, white, and rather tranſparent ſubſtance, not much unlike ſuet, in which ſtate it principally reſiſted the diſſolvent power of the water.

When the bone in general ſeemed ſufficiently cleanſed for drying, I found in one part ſome remains of this ſuety ſubſtance; but as I was unwilling to continue the whole in water any longer for the ſake of this ſmall portion, and thought beſides that in its drying I might get ſome inſight into its nature, I expoſed it to the open air, and was ſurprized to find that in the courſe of three days it was intirely diſſipated, ſcarcely a trace of it remaining, unleſs that, in the particular portion alluded to, the boney matter was of a more duſky colour than elſewhere.

I had now the ſatisfaction to find the attention and care beſtowed upon the bones during the maceration amply repaid by the ſingularities which they preſented.

The left *os innominatum* was perfectly free from any unnatural appearance in every part, even to its junction at the *symphysis* of the *pubis*; but there the line was drawn, and disease immediately began to shew itself through the whole of the right *os innominatum*, and to advance as it were from a superficial ulceration to excrescences in the greatest quantity.

It is remarkable, that the cartilage connecting the *ossa pubis* should be so complete a boundary to the disease; for though the external *lamella* was in all that part of the *os pubis* and *ischium* (particularly at the *ramus* of the latter) which united forms the *foramen ovale*, not the least deficiency is to be observed in the left *os pubis*. So interesting to the knowledge of the nature of this disease is it to observe that the extent of the tumor, which terminated exactly at this part, should likewise as exactly have limited its effects or consequences.

So far then as the external *lamella* of the *os pubis* and *ischium* was deficient, so far these bones presented that roughness and irregular loss of substances which is commonly denominated ulceration or superficial *caries*. The bottom of the *acetabulum* had likewise suffered in a similar manner: but over the whole of the *ilium*, both externally and internally, the *lamellæ* seemed very little injured, though covered by vast quantities of boney

matter branching out into various forms of different sizes, which a minute examination and careful attempt to separate ascertained to be mere, though firm, adhesions to the surface of the bone, and adventitious to the part on which they were found.

I am, &c.

Gloucester,
November 1, 1780.

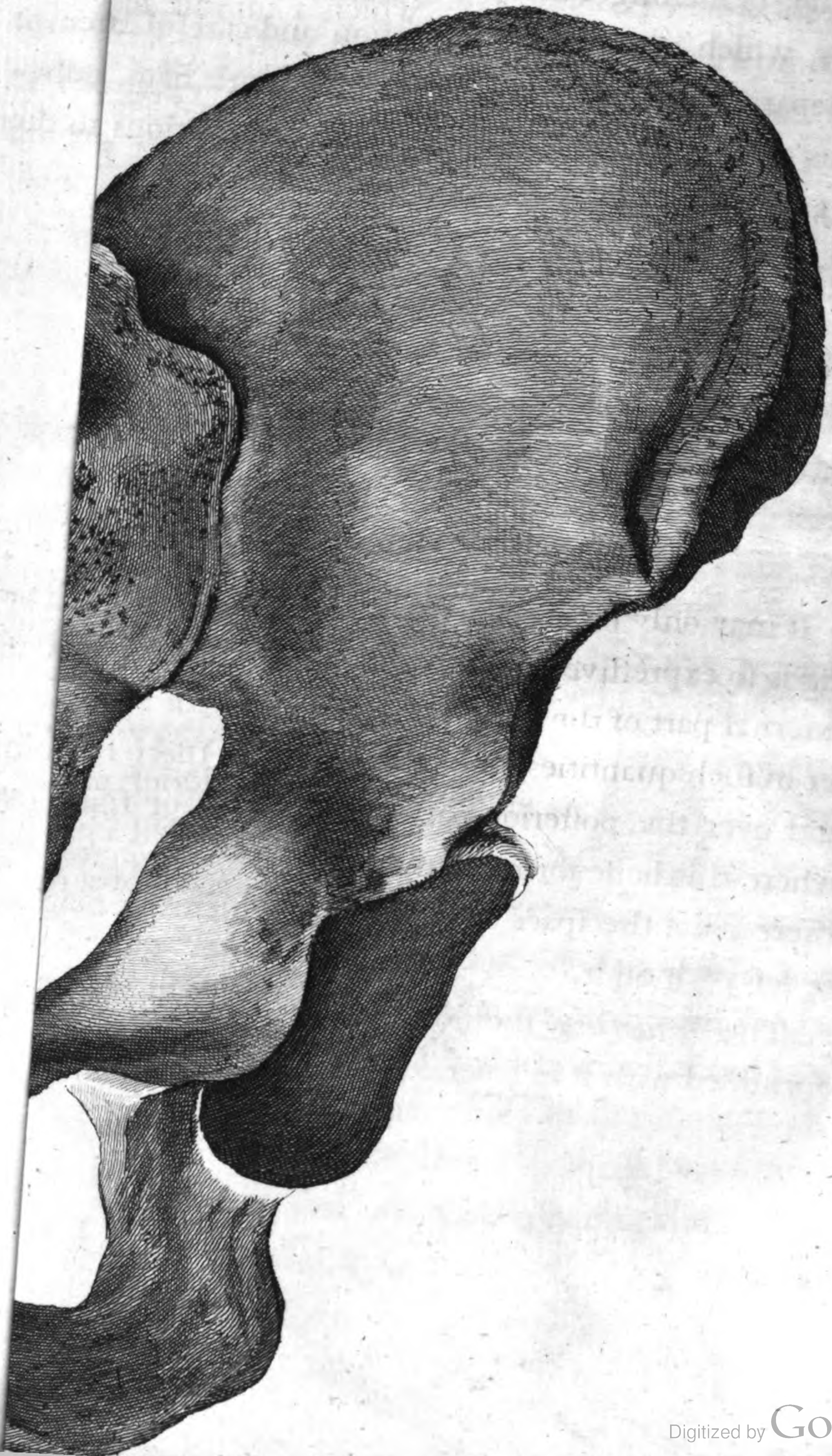
EXPLANATION OF THE DRAWINGS.

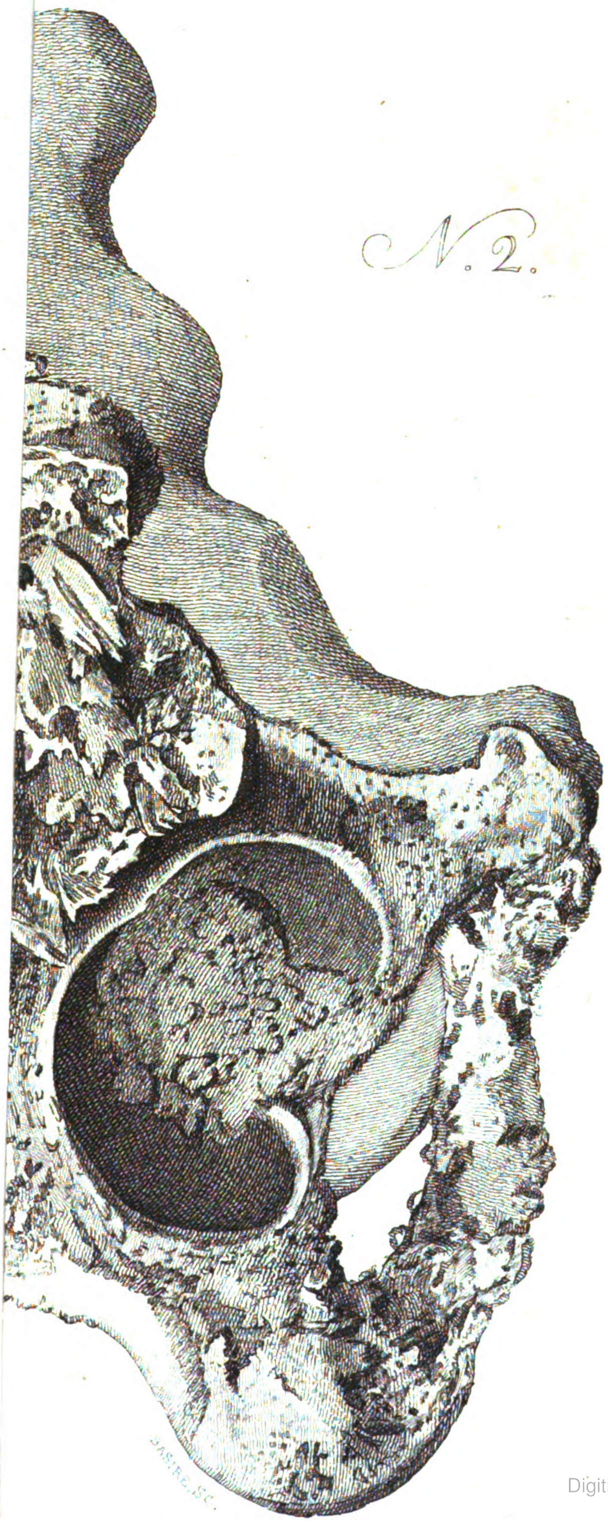
N° I. A front view of the *pelvis*.

It may only be necessary to observe here, as the drawing is so expressive of the original, that the centre of the external part of the *ilium* is not covered with boney matter in such quantities, or of such a size, as there is around and over the posterior part of the *crista* or spine, and where this bone forms a junction with the *os sacrum*; yet there is not the space of a six pence which has not more or less of it on it.

The *os sacrum*, though apparently sound, is likewise sprinkled with it in several places.

N°




N.° 2.

N° II. A fide view of the *pelvis*, fhewing,

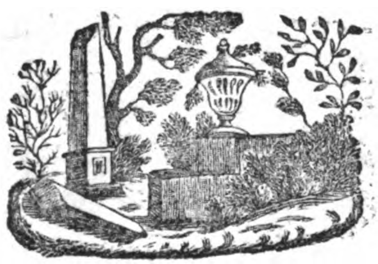
1. In the *acetabulum* a flight erofion.

2. The boney matter in different forms and fizes on the outward fide of the *ilium*.

3. The *os facrum* flightly expreffed, refting on a cork.

4. The *crifta* of the *ilium* at the pofterior part thickly covered with boney matter; at the anterior, free from it.

I have made no references in the drawings, as the above remarks will be fufficiently obvious.



THERMOMETRICAL EXPERIMENTS

AND

OBSERVATIONS.

By TIBERIUS CAVALLO, F. R. S.

Who was nominated by the Prefident and Council to profecute Difcoveries in Natural Hiftory, purfuant to the Will of the late HENRY BAKER, Efq. F. R. S.

Read at the ROYAL SOCIETY, June 15, 1780.

THERMOMETRICAL EXPERIMENTS, &c.

HAVING been appointed by the Prefident and Council of the Royal Society to write the annual differtation, purfuant to the inftitution of HENRY BAKER, Efq. F. R. S. I determined to prefent to the Society the following account of fome thermometrical experiments and obfervations, the greateft part of which were made as long ago as the year 1776. The courfe of thofe experiments having been often interrupted by mechanical defects in the conftruction of inftruments, and by various other circumftances, muft not be confidered as complete inveftigations of the propofed views, but rather as firft attempts, which are to be profecuted in cafe of better opportunities. They contain a few facts, which to me feem new and worthy of notice; and, perhaps, the obfervations concerning the conftruction and ufe of the inftruments ufeful in experiments of this fort, may be of ufe to thofe perfons who have the opportunity, and are willing, to profecute this experimental inquiry.

Having read in a volume of the Philofophical Tranf-actions the account of an experiment made with a thermometer, whofe bulb was painted black, and was expofed to the rays of the Sun, in which cafe it had been found, that the thermometer fhewed a much greater degree of heat than when not blackened, I was defirous of trying the ultimate limits of this difference. For which purpofe I conftructed two thermometers, the fcales of which (being made by trial) coincided fo perfectly well together, that when the thermometers were put in equal circumftances, no difference could be perceived between the degrees of heat fhewn by them. The method ufed to graduate thofe thermometers is as follows. The fcale of one of the thermometers was made in the ufual manner, *viz.* by finding the boiling and freezing water points, and dividing that diftance into 180 equal parts, which are the degrees according to FAHRENHEIT. Here no regard was paid to the barometrical height at the time of finding the boiling water point, it being ufelefs for the experiment for which the inftrument was intended. This done, the balls of the two thermometers were put into hot water, and according as the water cooled, and confequently the mercury defcended in the tubes, different marks were put upon the tube of the ungraduated thermometer,

which were nominated after the degree of heat ſhewn at the ſame time by the graduated thermometer. Thus, for inſtance, a mark was put at 140°, another mark was put at 135°, another at 130°, and ſo on. It is plain, that in thoſe points, the thermometers when put in equal circumſtances, muſt coincide perfectly together. Now, on making the ſcale thoſe points are marked firſt, and as the diſtances between thoſe marks were very ſmall (conſiſting of a few degrees) they were divided with the compaſſes in the proper number of equal parts or degrees; and in this manner the ſcale of the other thermometer, was completed, by which means, although theſe two thermometers did not coincide ſo well with other thermometers, yet they coincided perfectly well together, as muſt inevitably be the caſe, even upon the ſuppoſition that their tubes were not perfectly cylindrical. The length of a degree on the ſcale of thoſe thermometers was a little more than $\frac{1}{20}$th of an inch, and although thoſe ſcales were divided into degrees only, yet by inſpection a perſon a little verſed in theſe obſervations could eaſily diſtinguiſh the height of the quickſilver within a quarter of a degree.

Theſe thermometers were both fixed upon the ſame frame at the diſtance of about one inch from one another, having the balls quite detached from the frame, and in this manner they were expoſed to the Sun, or to the light of a lamp.

 When

When theſe thermometers were expoſed to the Sun, or kept in the ſhade, they ſhewed the ſame degree preciſely. The difference between the degree ſhewn by theſe thermometers when expoſed to the Sun, and when kept in the ſhade at about the ſame time of the day, was very trifling.

When the ball of one of thoſe thermometers, which we ſhall call A, was painted black with Indian ink, or with the ſmoke of a candle, and that of the other thermometer B was left clean, on being expoſed to the Sun they ſhewed different degrees of temperature; the quickſilver in the tube of A was much above the quickſilver in the tube of B. This difference ſometimes amounted to about 10°. but it was never conſtant, varying according to the clearneſs of the Sun's light as well as of the air, and alſo according to the different degrees of temperature of the atmoſphere.

Keeping the frame with thoſe thermometers, one of which had the ball painted black, hung on the ſide of a window, I obſerved a remarkable fact, *viz.* that theſe thermometers ſhewed unequal degrees of heat, not only when preſented to the Sun, but alſo when expoſed to the ſtrong day-light. I cleaned the bulb of the thermometer A, and blackened that of B, but the effect was conſtant, *viz.* the quickſilver in the tube of the thermometer, whoſe

whoſe bulb was painted black, was conſtantly higher than the other, whenever they were expoſed to the ſtrong day-light. This difference was commonly about one-third of a degree, but ſometimes it amounted to three-fourths, and even to a whole degree. The ſituation in which thoſe thermometers were uſually placed was ſuch that the light of the Sun could not be reflected upon them by any object ſtanding before; but the experiment anſwered even when the Sun was hidden by clouds.

This obſervation ſeemed to ſhew that, perhaps, every degree of light is attended with a proportionate degree of heat; and induced me to try, in a ſimilar manner, whether, by directing the concentrated light of the Moon upon the blackened ball of one of theſe thermometers, I could render ſenſible the effect of that light [(a)]. But although I attempted it ſome time ago with a large lens ſeveral times, and have lately tried it again with a burning mirror of eighteen inches diameter, yet ſometimes for want of proper means of obſerving the height of the mercury in the tubes of the thermometers, ſometimes for want of a continued clear light of the Moon, and in ſhort from one unfavourable circumſtance or

(*a*) The concentrated light of the Moon has often been thrown upon thermometers without any effect; but I do not know that any blackened thermometer was ever uſed before for this purpoſe.

other,

other, I have not yet been able to make a fair and decifive trial of this experiment.

The light of the Sun being very inconftant on account of clouds and of its diurnal motion, I thought to make fome experiments with the above mentioned two thermometers, by expofing them to the light of a lamp, and I found that this light had a confiderable effect upon them.

The ball of one of the thermometers being blackened, and both being fet at two inches diftance from the flame of a lamp, they both rofe from 58°, at which the mercury ftood before the lighting of the lamp, to 65°$\frac{1}{2}$, and the blackened thermometer to 67°$\frac{1}{2}$. Another time, being fet at the fame diftance from the lamp, the uncoloured thermometer came up to 67°$\frac{3}{4}$, and the blackened one to 68°$\frac{3}{4}$. In fhort, by various repeated trials it appeared, that the difference generally amounted to about 1°. When the thermometers were put farther than two inches from the lamp, this difference decreafed, and at about fourteen or fifteen inches it vanifhed quite.

It is mathematically true, that emanations which proceed from a center, and expand in a fphere, muft continually become more and more rare in proportion to the fquares of the diftances from the center. Thus it is faid, that the intenfity of light proceeding from a luminous body

body at the double, treble, quadruple, &c. of a given diſtance from that body, muſt be reſpectively four, nine, ſixteen times leſs denſe. The ſame thing may be ſaid of heat.

Being willing to aſcertain this truth by actual experiment, I placed ſeveral thermometers, whoſe balls were not painted, at different diſtances from the flame of the lamp, and expected to find, when the thermometer at four inches diſtance was 1° above that placed at eight inches diſtance, the thermometer placed at two inches diſtance ſhould be 4° higher. But upon trying this experiment various times, placing the thermometers at different diſtances from the flame of the lamp, and making the proper calculations agreeable to thoſe diſtances, it appeared, that the intenſity of the heat did not decreaſe exactly in the duplicate proportion of the diſtances from the flame of the lamp, but ſhewed a very odd irregularity. It ſeemed to decreaſe faſter than the duplicate proportion of the diſtances for the ſpace of two inches and a half or three inches, after which it decreaſed much ſlower. Whether this effect may be attributed to ſome different ſtate of the air's purity at different diſtances from the flame of the lamp, or to the vapours proceeding from the flame, I cannot take upon me to determine.

The

The above mentioned experiments gradually induced me to try the effect of the light of the Sun and of a lamp upon thermometers whose balls were painted with different colours. Dr. FRANKLIN's experiment with the pieces of cloth set upon snow that was exposed to the Sun is very well known. The doctor found, that those pieces of cloth, whose colour was darker, sunk deeper in the snow than the others, by which it appears, that they became hotter. My view was to examine those different degrees of heat imbibed by different coloured substances with precision, in order to observe if they kept any proportion to the spaces occupied by the prismatic colours in the prismatic spectrum, or if they followed any other discoverable law; but those attempts met with many difficulties, the greatest of which was the choice of colours. The water colours that are commonly used, as carmine, sap-green, &c. are of so different a nature from one another, that when the balls of the thermometers were painted with them, their surfaces were not equally smooth, which occasioned great difference in the effect; for I found, that two thermometers, whose balls had been painted with the same colour, but the paint laid smoother on one than on the other, shewed different degrees of heat when they were both exposed to the rays of the Sun.

I attempted

I attempted to make thermometers with tubes of differently coloured glafs, but when a ball was formed with any of thofe tubes, the fubftance of the glafs in the ball, being much thinner than in the tube, differed very little from clear colourlefs glafs.

To include the thermometers in clofe boxes, in which the rays entered through coloured glaffes, was alfo found ineffectual; not only becaufe the colours fo tranfmitted were far from being homogeneous, but efpecially becaufe fome of thofe glaffes are much more opaque than others, even of the fame colour.

The leaft ambiguous method, therefore, was that of painting the balls of the thermometers with water-colours, taking care to lay them as equally fmooth as poffible. In this manner I repeated feveral experiments, ufing fometimes a dozen of thermometers at once, whofe balls were painted with various colours, and were expofed to the Sun; and from a vaft number of experiments, and after fome weeks obfervation, it could be only deduced, that if the colours, with which the balls of the thermometer were painted, were pretty like the prifmatic colours, thofe thermometers fhewed a greater degree of heat, whofe colours were nearer to the violet in the order of the prifmatic colours, and contrarywife; but they were all, even that painted with white lead, in fome

intermediate degree between the blackened thermometer and the naked or unpainted one. If the colours had not the proper degree of denſity, the effects were very different: thus a thermometer painted with a light blue was lower than another thermometer painted red with good carmine.

I ſhall now deſcribe the manner of conſtructing the ſcales of thoſe thermometers, which was contrived ſo as to be very expeditious; becauſe ſome of thoſe thermometers were often broken by ſome accident or other, and that new ſchemes often required new thermometers, to conſtruct the ſcales of which in a formal manner would have required a very long time. Thoſe methods therefore may be of uſe to other perſons.

When the thermometers were intended to be expoſed to the flame of the lamp, at a given diſtance from it, their ſcales were drawn upon ſlips of paper which were glued to their tubes in the manner repreſented by fig. 1. The thermometers were then ſet horizontally upon a book, ſo that their balls were out of the book, and at any required diſtances from the flame of the lamp, which diſtances were meaſured with a pair of compaſſes. But when the thermometers were only to be expoſed to the Sun, I then uſed the following very expeditious method, ſee fig. 2. Upon an oblong board ABCD of about 14 inches by 18,

 and

and nearly one inch thick, I paſted a piece of white paper, and delineated upon it a right-angled triangle EGF, one ſide GF of which came very near the edge CD of the board; the other ſide EF, which ſtands perpendicular to GF, was divided into equal parts, repreſenting degrees of FAHRENHEIT'S thermometrical ſcale. The loweſt of thoſe degrees was near the freezing point, and the upper was not above the 110°, it being as much as I was in want of. From the point G right lines were drawn to all the degrees in the ſcale EF, and many other right lines parallel to the ſcale EF, and conſequently perpendicular to the baſe GF, were drawn through the whole area of the triangle. Now when the thermometers were conſtructed, I found, by compariſon with a ſtandard thermometer, two points whatever, which I marked with the file upon the tube, and by a note of theſe thermometers I knew to what degrees they anſwered. Thus, for inſtance, upon the thermometer HL two marks were made, *viz.* I anſwering to the 40th degree, and K to the 70th degree. Now when this thermometer was to be uſed I placed it upon the board ABCD with the tube parallel to the ſcale EF, which could be eaſily done by the help of the parallel lines drawn upon the triangle, the ball of the thermometer being out of the board; thus I ſlid the thermometer backwards and forwards till the mark I,

 which

which was the 40th degree, coincided with the line, which went from the point G to the 40th degree on the ſcale, and the mark K coincided with the line which went from G to the 70th degree. In that place the thermometer was left, and in order to keep it ſteady, a piece of lead (O) was uſually put upon the extremity of its tube. In this ſituation the interſections of the tube, and the lines drawn from G to the degrees in EF, ſhewed the degrees, or ſerved for a ſcale to the thermometer HL. Thus, ſuppoſe that the quickſilver in its tube was at P, it is plain, that it was at 55°, becauſe at that point the tube is interſected by the line which goes from G to the 55th degree on the ſcale EF. This board, therefore, ſerved for a univerſal ſcale, and upon it I uſed to fix ſeveral thermometers at a time, and expoſe them all together to the Sun.

I ſhall conclude this paper with mentioning an experiment, which, although not thermometrical, is yet uſeful in removing a wrong notion ſome perſons have concerning the effect of light.

Having ſeen in ſome book that the common black pyrophorus, or HOMBERG's pyrophorus, was impaired by light, I was deſirous to try the truth of this aſſertion. Accordingly towards the beginning of the laſt year I prepared ſome pyrophorus, and incloſed portions of it in three glaſs tubes, which were immediately ſealed

2 hermetically,

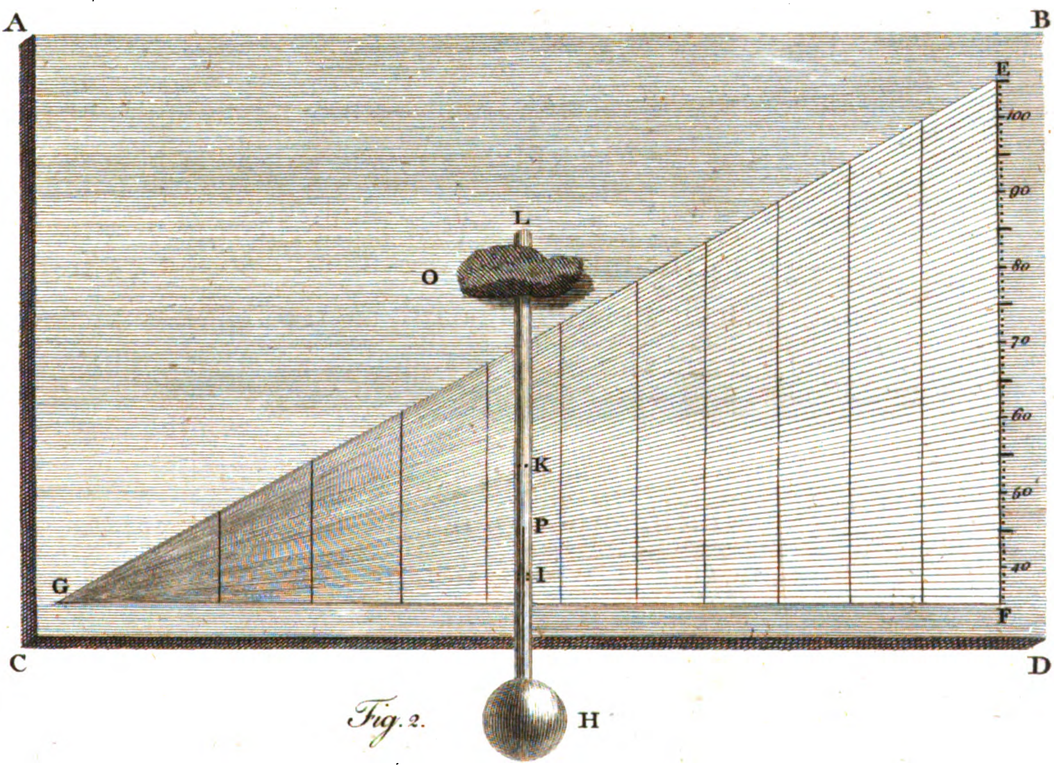


Fig. 2.

hermetically, and on the 20th of May, 1779, two of them were ſuſpended to a nail out of a window, and the third was wrapped up in paper, and was incloſed in a box, where not the leaſt glimmering of light could enter. In this ſituation they were left for above a year, and laſt week I broke one of thoſe that had been kept out of the window, and that which had been in the dark, in preſence of Mr. KIRWAN, F. R. S.; but the pyrophorus of each tube ſeemed to be equally good, taking fire within about half a minute after being taken out of the tubes, and expoſed to the air upon pieces of paper, which ſhews that neither the preſence or abſence of light had injured it.

June 7, 1780.

P. S. Having mentioned to ſeveral perſons my intention of making ſome experiments upon the temperature of the atmoſphere with a new metalline thermometer, and of giving an account of them in this paper, I muſt here mention, that as the inſtrument was not finiſhed in proper time, I ſhall defer giving a deſcription of it, as well as an account of the experiments, &c. to another opportunity.

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A D D E N D A.

At the end of page 386 *add*,

In fig. 4. draw a circle through the points N, M, T; and at the maximum where $TP^2 = PM \times NP$ this circle will touch CA produced in T. From E the center of this circle draw EF perpendicular to NM, also the radii EN and EM; and FN is the sine of NEF, or half NEM, or of its equal MTN, to the radius EN. But $EN = ET = PF = \frac{PN+PM}{2}$, and $FN = \frac{PN-PM}{2}$. Therefore $PN + PM$ is to $PN - PM$, or $CD + CB$ is to $CD - CB$, or $CA + CB$ is to $CA - CB$, as radius is to the sine of the greatest angle of deviation, which is therefore equal to $\frac{CA-CB}{CA+CB}$, radius being unity.

E R R A T A to Vol. LXX.

Page 6, *line antepenult. read* be nearly mathematically.
6, *l. penult. dele* yet.
7, *l.* 13, *dele* section ABC, or
7, *l.* 18, *at the end of the line add* very nearly.
394, *l.* 15, *transpose* general equation *to the beginning of the line above.*
402, *l.* 6, 7, 8, *for* 9143 *r.* 9443.
405, *l.* 7, *for the last*, $1+\sqrt{-3}$ *r.* $1-\sqrt{-3}$.
405, *l.* 11, *for the last* $-\frac{\sqrt{-3}}{2}$ *r.* $+\frac{\sqrt{-3}}{2}$.
443, *end of the 1st line, for* and x *r.* and X.
548, *l.* 10, *for* circumstances *r.* and which are.

*** There are FIFTEEN Plates in this Volume